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THE GROSVENOR GALLERY.

A FOREIGNER who should take a hasty glance through the Grosvenor Gallery might at first suppose it was intended as a place of refuge for works of art (Heaven save the mark!) which elsewhere would be regarded as insults to the public. Looking more carefully through the collection, he would find many charming works manifestly placed here for their excellence, not for the grossness of their defects. It would probably remain a mystery to him how works so unlike in character came to be side by side in the same collection, did not some English friend tell him of the idiocies of the æsthetic school of art, and of the insolent madness of that school of which Mr. Whistler is the most peccant—we wish we could say, the only—representative.

In the early youth of painting (passing over the Egyptian, Greek, and ancient Roman schools, of whose works we know very little), artists had crude ideas of drawing and colouring, they knew very little of perspective and anatomy, and they had scarce any models worth copying. They had, as children have, a taste for bright colours; and they possessed what children, as a rule, do not possess, the power of depicting beautiful colours, though in inharmonious combinations. Thus we find in their paintings imperfect perspective, angular figures, impossible bones and muscles, unwholesome complexions, and gaudy ornamentation—but, occasionally, very beautiful tints. Of aereal perspective the early painters knew simply nothing—or, if they knew anything, they made not the slightest attempt to apply their knowledge.

The paintings of the early schools are therefore necessarily defective, and so far as general effect is concerned, they are, for the most part, in the artistic as well as ordinary sense, simply hideous. But they are full of interest. Moreover, scarcely one of the paintings by the ablest of the early painters fails to show here and there features which, considered alone, are of exquisite beauty. The charm of antiquity appeals to us as we study these ancient paintings. The practised eye can see in them also the germs of the noble works of the great painters who came afterwards. In every national collection these old paintings have a place of honour; every student of art examines them with loving care; and if occasionally there is something of the feeling with which we look over a portfolio of pictures by a child, if there is sometimes a sense of amused wonder at the quaintness of the conceptions and the inadequacy of their rendering, the feeling is softened, much as when, looking at pictures by a child who is dead, the thought comes to us that his tiny fingers never learned to hold pencil or brush with firmness and vigour, that he was never more than the child-artist.

And because men of sense and of artistic skill study with love and tenderness these quaint old beginnings of modern art, there "come you in" certain idiots, hoping to beguile us of our esteem by copying the defects of those beloved old masters who flourished during the childhood of painting. As if some inane noodle of middle age should take the grotesque drawings of a child, and copy all their absurdities, these imbeciles of the present day (when art, though it should at least have reached its full manhood, ought not to be in second childhood) carefully picture sickly, ill-shaped beings, in impossible attitudes (they call them mediæval, but people in the middle ages were not all crippled and deformed), adorned with tawdry, ill-arranged frippery, and wanting even what the quaint old pictures possessed, occasional touches of beautiful colouring, and occasional bits of effective drawing.

Take, for instance, "The Feast of Peleus," by E. Burne Jones, No. 157 (East Gallery). For what earthly reason is a long-suffering public to be insulted by a picture in which the Laughter-Loving Goddess of Beauty is represented as a high-shouldered, hideous skeleton? and in which the two rival goddesses who claim the apple (marked by Discord "for the fairest!") are as ugly and dyspeptic as their dreary rival?—uglier they could hardly be. There was some excuse for a Ghirlandajo or a Botticelli who represented a saint as one who had lived an ascetic life—in other words, as a pale, half-starved weakling. But there is not common sense or even decency in picturing these wan and wasted wretches as "the three great goddesses who claimed the golden apple as the prize of beauty." In the "Tree of Forgiveness," by the same (No. 144) we are pained perhaps more by the offensiveness of the subject (in an artistic sense) than by the hideous complexions and expressions (we say nothing about the utterly incorrect proportions of Demophoon's chest, belly, and limbs, seeing that the painters of this school *must* be all wrong anatomically, or they would not be mediæval). A lady who had been an almond tree for a while might have such a colour (for aught we know), and a man upon whom such a creature suddenly sprang out might be excused for wearing a very uncomfortable expression. But Venus Aphrodite sick and sorry, worn, wan, and wasted, we really "cannot away with." Of the three ladies in "The Mill" (No. 175), by the same painter, we can only say that they are suggestive rather of stale rhubarb than of faint lilies; and a similar remark applies to the Angel with complicated wings in No. 292, whose melancholy expression by no means accords with the usual ideas respecting angelic happiness.

It is, indeed, easy to fall into a way of lightly ridiculing this absurd school. But their offences merit more serious chastisement than mere ridicule. The mischief such paintings do is very serious. Among the inexperienced they create utterly false tastes. They are not only bad in themselves: they are bad in their indirect influence. They kill men's love for the works of the old painters. If there are any who have done more than others to destroy our appreciation of those works, to make them positively hateful and disgusting to us, instead of interesting and delightful (as rightly viewed they should be), it is their so-called followers, who admire them for their defects, carefully

copy their defects, and have nothing in common with them but the worst of their defects.

But if we feel contempt for the paltry affectation of the old style, with what feelings must we regard the mad new style, the Nocturnes in Blue and Silver, the Harmonies in Flesh-colour and Pink, the Notes in Blue and Opal? We are shown a sooty-faced, ill-shaped creature, with limbs entirely out of proportion (do *look* at that left arm!), on a dirty-black background, with smears of vermilion on necklace, lips, and hat, and we are told it is—not a Horror in Soot and Ochre, as our eyes tell us—but (forsooth) a Harmony in Black and Red! A dark bluish surface, with white dots on it, and the faintest adumbrations of shape under the darkness, is gravely called a Nocturne in Black and Gold. A few smears of colour, such as a painter might make in cleaning his paint-brushes, and which neither near at hand nor far off, neither from one side, nor from the other, nor from in front, do more than vaguely suggest a shore and bay, is described as a "Note in Blue and Brown," and purports to present that well-known and lovely spot, St. Brelade's Bay. Criticism is powerless here, because one who found these pictures other than insults to his artistic sense, could never be reached by reasoning. We are not sure but that it would be something like an insult to our readers to say more about these "things." They must surely be meant in jest; but whether the public have chiefly to thank Mr. Whistler, or the managers of the Grosvenor Gallery, for playing off on them this sorry joke, we do not know, nor greatly care.

Meliora canamus!

(To be continued.)

ENGLISH SEASIDE HEALTH-RESORTS.

By Alfred Haviland.

CLASSIFICATION.

BEFORE describing the seaside towns usually resorted to for health or change, either singly or in groups, it is necessary that they should be classified in such a manner as to assist the reader in forming a correct idea of their general and social climatic differences; for, when this part of the subject is clearly understood, health-seekers and their advisers will be less likely to err in the too-frequent direction of expecting from certain health-resorts what Nature has never given them to bestow, and will be more likely to find what they want, when, after first ascertaining what their bodily conditions absolutely require, and what, from their hereditary tendencies or other peculiarities of constitution, must be carefully avoided, they study each group and each member of it, with all the knowledge they possess of its characteristic climatic properties, and, after doing so, select such a resort as will fulfil their health-requirements, without evoking latent tendencies to disease—a most important point to be considered, although one unfortunately too frequently neglected.

Health-resorts must be studied—I. As to latitudinal and longitudinal position; for the English coast, including that of Wales, is between 6° 1′ 45″ degrees of the former, and 7° 11′ 55″ of the latter, and thus stretches over an area including a remarkable variety of climatic factors. As regards latitude, the most southerly point is the Lizard Head, in the district of Helston, Cornwall, 49° 36′ 35″ lat. N.; the most northerly, Berwick-upon-Tweed, in Northumberland, 55° 38′ 20″ lat. N. As to longitude, the most easterly point is Lowestoft, in the district of Mutford, Suffolk, 1° 31′ 25″ long. E.; and the most westerly, the Land's End, in the district of Penzance, Cornwall, 5° 40′ 30″ long. W.

II. As to their position on the seaboard; whether they enjoy the warm moist air from the Gulf Stream, the bracing air of the North Sea, or an atmosphere brought by the winds after it has lost its sea character by passing over thousands of square miles of continent.

III. As to their site on the coast; whether they are elevated on precipitous cliffs, having a protective influence, or on low-lying, flat shores, over which the winds find no resistance; or in deep valleys facing the sea, where the natural force of the wind is increased, like that of the tidal wave, in funnel-shaped estuaries; as to the heights behind them, whether, although they may be protected by them from some winds, these heights do not act injuriously, on the whole, by preventing due air flushings when the winds from the sea are only moderate in force, especially in the case of large towns where there is much smoke.

IV. As to the depth or shallowness of the sea; whether the beach offers opportunities for exercise and recreation, or whether at low tide there is a wide expanse of mud exposed.

V. As to their exposure to or protection from certain prevailing winds, and whether their street arrangement is such as to afford the health-seeker opportunities of availing himself of natural advantages.

VI. As to the prevalence or scarcity of certain diseases in the resorts themselves and in the neighbouring districts, and as to the zymotic death-rate.

VII. As to the geology and physical geography of the site and its neighbourhood.

VIII. And, as far as practicable, as to the temperature, its monthly mean, daily range, its mean monthly maximum, minimum, and range, the rainfall, and, wherever it can be ascertained, the temperature of the sea.

The above is an outline of what we hope to present to our readers, and we need hardly say that we shall be grateful to all who will kindly assist us with reliable information on the above or any other points of interest connected with the seaside health-resorts of England. We will now give a list, under our first heading, of some of the resorts which will more or less command our attention in future papers.

I. Latitudinal Position. On the East Coast.—Between 51° and 52° N., Deal, Ramsgate, Margate, Herne Bay, and Southend. Between 52° and 53° N., Lowestoft, Great Yarmouth, Cromer and Huntstanton. Between 53° and 54°., Great Grimsby. Between 54° and 55° N., Bridlington, Filey, Scarborough, Whitby, Redcar. On the West Coast.—Between 50° and 51° N., Sennan (Land's End), St. Ives. Between 51° and 52° N., Ilfracombe, Burnham, Weston-super-Mare, Clevedon, and Tenby. Between 52° and 53° N., Aberystwith and Bournemouth. Between 53° and 54° N., Beaumaris, Bangor, Penmaenmawr, Llandudno, Rhyl, New Brighton, Southport, and Blackpool; and between 54° and 55° N., Morecombe Bay. On the South Coast.—Between 51° and 52° N., Dover, Folkestone, and Hythe; and between 50° and 51° N., Hastings, and St. Leonards, Eastbourne, Seaford, Brighton, Worthing, Littlehampton, Bognor, Isle of Wight, Bournemouth, Weymouth, Lyme Regis, Exmouth, Dawlish, Teignmouth, Torquay, Dartmouth, Penzance, and the Lizard.

The reader will kindly correct the following errata in the first paper, p. 3, col. II. For "seasonably," read "seasonally"; after "distributed" insert "in"; for "teach," read "teaches"; and "before such places" omit "that."

The "Christian Commonwealth" has been permanently enlarged from 16 to 24 pp.

ANTIQUITY OF MAN IN WESTERN EUROPE.

BY EDWARD CLODD.

PART III.

SCANTY as are the bones of Palæolithic man, no unprejudiced person can deny that the tools and weapons of the Drift are products of human skill, low in the scale as this may be; for they have defined, purposeful shapes, which were artificially produced, because they can be thus formed only by the application of blows or of pressure in a peculiar way, as modern experiments show. Moreover, they indicate selection on the part of their fashioners, since they cannot be made from every kind of flint. They are found, in striking correspondence of form, wherever man is known to have wandered, or may be presumed to have wandered, over the earth*—in the alluvials of the East, the laterite or brick earth of Madras; in the river-gravels of sacred and classic lands, by the Sea of Galilee, and along the valley of the Tiber; in brief, in every explored part of the Old and New Worlds "from China to Peru." They witness to the wide distribution of rude tribes of hunters in the lowest stage of culture, of whose aboriginal home we can only speculate, of whose ultimate fate nothing whatever is known.

> Into this Universe, and *why*, not knowing,
> Nor *whence*, like water willy-nilly flowing;
> And out of it, as wind along the waste,
> I know not *whither*, willy-nilly blowing.†

The description of rude Northern tribes given by Tacitus may not unfitly—rather with added force—be applied to them. "They are wonderfully savage and miserably poor. They have no weapons, no horses, no homes; they feed on herbs, and are clad with the skins of beasts; the ground is their bed, and their only hope of life is in their arrows, which, for lack of iron, they sharpen with tips of bone. The women live by hunting, just like the men, for they accompany the men in their wanderings and seek their share of the prey. And they have no other refuge for their young children against wild beasts or storms than to cover them up in a nest made of interlacing boughs. Such are the homes to which the young men return, in which the old men take their rest."‡

The implements of the ancient Stone Age are certainly not more recent than the water-laid beds in which they are found hitherto undisturbed, and the height of these, ranging from 50 ft. to as much as 200 ft., in some places, above the present river-levels, is proof of enormous antiquity. An approximate estimate of the time required for the deepening of a valley is furnished by the quantity of sediment carried yearly by the river flowing through it to the sea. The data at hand for this result are slender, but Professor A. Geikie shows that the removal of one foot of rock occupies in some cases a few centuries; in others as many chiliads. For example, the Po appears to lower the surface of the area drained by it at the rate of one foot in 729 years, while the Mississippi takes 6,000 years to effect a like result. And although the larger volume and flood of the Pleistocene rivers—betokened by the coarse gravel, the large unrolled stones, and the mingled remains of different species of animals which alternately occupied the land, as the climate of Pleistocene times was now genial, now arctic—scooped out the valleys at a quicker rate than the rivers of to-day, the removal by the Somme, for example, of masses of chalk and overlying Tertiary *débris* (of which its valley entirely consists) to the depth of 150 ft., through a channel many miles in length, demands an immense period. Subsequent to this, what vast lapse of time is required to explain the gap between the Drift and the early pre-historic period when the polished stone-using peoples arrived, so that, as Dr. Evans remarks: "we must, for the present at least, judge of the antiquity of these deposits rather from the general effect produced upon our minds by the vastness of the changes which have taken place, both in the external configuration of the country and its extent seaward, since the time of their formation, than by any actual admeasurement of years or of centuries."*

But these implements of the river gravels do not tell the whole story about Palæolithic man. Speaking broadly, he falls into two divisions—Drift-man and Cave-man; the tools and weapons found in the limestone caverns of Western Europe marking a distinct advance, perchance due to another race, over those of the earlier period. For while, as has been remarked, the gravel-beds yield only oval-shaped flints and leaf-shaped flakes, the caves furnish flint saws, lance heads, awls, barbed weapons, bone needles (in one place a stone drill lying near them), imbedded with charcoal and the *débris* of animals eaten, as the musk sheep, bison, and others, especially the reindeer. From the enormous numbers of this creature, which appears to have formed the chief food of the rude hunters, the cave deposits are often spoken of as belonging to the Reindeer period, in contradistinction to the Drift or Mammoth period. The precedence of decoration over dress noticeable among savages, perhaps finds illustration in rude strings of animals' teeth and shells; while a soft red ochre (oxide of iron), which occurs among the relics, shows that the Reindeer men painted their skins. But we are anticipating.

From the earliest times, "the clefts of the valleys, the caves of the earth, and in rocks," have formed the natural shelters of barbaric races. And, although the traces of bone and other relic-yielding caverns of tertiary and early quaternary times appear to have been swept away in the momentous changes of land surface, the caverns of subsequent periods are rich in remains which enable us to construct a more vivid outline of man in the ancient Stone Age than do the scanty and rude relics of the Drift. Implements of the Drift type occur among the oldest layers in caverns, but the upper deposits supply the evidence of advance to which reference has been made. Following M. de Mortillet's divisions for a moment, the Thenaisian and Acheulian epochs embrace the Drift period; the Mousterian, Solutrian, and Magdalenian epochs the Cave period. Not that these latter divisions are to be taken as hard and fast, but as overlapping, because they may, in fact, represent no great difference or extended succession of time. And for the present purpose it is better that in place of explanation of, or comment upon, the method of selecting specific implements as types of each period adopted by M. de Mortillet, an account be given of the deposits and contents of one of our most celebrated English caverns, premising that these will afford a fair idea of the bone-caves and rock-shelters of the continent, except where the latter have furnished certain remarkable specimens of primitive art to be presently referred to.

Before entering our cave, which shall be Kent's Hole, near Torquay, let us briefly explain how the deposits in it, and in like caverns found in limestone rock, have been

* Scandinavia must be excepted, the finds there being exclusively Neolithic.

† Rubáiyát of Omar Khayyám, xxix. ‡ Germania, c. 46.

* "Ancient Stone Implements," p. 621.

formed, because the almost imperceptible rate at which, in some cases, they have been laid down has an important bearing on the age of the contents imbedded within them. The surface-water finds its way through some crevice or fissure in the rock, and being more or less charged with carbonic acid derived from the atmosphere, and from decayed vegetable and other matter in the soil, becomes a very powerful solvent, under which the rock, converted into carbonate of lime, is carried away in solution. But while much thus runs off, Nature, true to her cyclical action, begins to refill the cavity which she has eaten away. Drops of water, holding the lime in solution, ooze from the roof, and, falling on the floor, gradually form beds and bosses of stalagmite, which hermetically seal whatever *débris* may happen to be lying about, while such portions of the dissolved limestone as remain clinging to the roof after evaporation of the water from them hang icicle-like therefrom, and gradually form the beautiful columns of stalactite which adorn many of the famous caverns of the world.

CRYSTALS.

By William Jago, F.C.S., Assoc. Inst. Chem.

No. III.

IN our last paper we studied crystals, artificially prepared, under the microscope; it is our purpose in this number, still using the microscope as our means of investigation, to examine crystals which have been formed naturally. From what has already been stated, it will be seen how universal is the tendency which matter has, when assuming the solid state, to also become crystalline. Rocks form no exception to this rule, and in many specimens of the granitic type, large and well-formed crystals are found embedded in the finer portions of the rock. The name "porphyry" is now restricted by geologists to rocks of this class. The stone of the large polished granite fountain in St. Paul's Churchyard contains some very fine crystals of the minerals felspar and hornblende; but not only is granite distinctly made up of crystals, but even the finer grained rocks, which to the naked eye appear perfectly uniform in texture, are also found on minute examination to be more or less crystalline in structure. Of recent years the application of the microscope in geology has been much extended, and now the description of any rock would not be considered complete without mention of its microscopic characteristics. To examine rocks with the microscope, by means of transmitted light, in the way most usually adopted, it is necessary to prepare and mount extremely thin sections, these sections being often much thinner than paper. At present we shall not be able to give directions for the preparation of such specimens, but, with the Editor's permission, may make practical directions for section-grinding and mounting the subject of a separate paper on some future occasion.

In the first paper of this series it was stated that the black, pitch-like form of lava is instanced by the geologist as the type of glassy or non-crystalline rocks. Fig. 1 is a sketch made of such a piece of glassy lava, "obsidian" from the Lipari Isles, magnified to about 250 diameters. It is almost a pure natural glass; the small black specks are probably embryonic crystals of magnetite (one of the natural oxides of iron). Such a rock is produced by the rapid cooling and solidification of molten rock material; with slower cooling, "obsidians" are formed in which a decided step further in advance towards crystallisation is made. Fig. 2 represents a spherulitic obsidian from Iceland. (This, and all the succeeding figures, are magnified from about 25 to 30 diameters.)

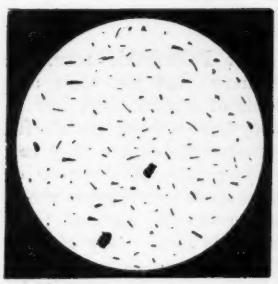


Fig. 1.

Fig. 2.

Although the main portion of the rock is glassy, there are yet a few small crystals to be noticed; the most interesting point, however, about the specimen is that it contains a number of small spherules, one of which occupies the centre of the field. It may be noticed that it has a radiated structure. Precisely the same formation is often seen in glass which has been maintained at a temperature near its melting-point for a considerable time. The writer has several interesting examples of such glass in his possession. But, to proceed to some further proof of the crystalline nature of these spherules. Students of physics are aware that a ray of light undergoes a peculiar change on passing through a block of Iceland spar; among other modifications, it is so altered that it is incapable of passing through a second block of the same material if placed in a certain position with regard to the first. The light under these circumstances is said to be *polarised*. Microscopes for the study of rock sections are fitted with a pair of specially-prepared pieces of Iceland spar, called "Nicol's Prisms." One of these is fixed under the stage; the other is either fitted over the eyepiece, or into the body of the instrument. These being placed in position, on rotating one of them, a point is reached at which, on looking through the instrument, it is seen that no light is transmitted. Most crystalline substances, on being placed between the prisms, possess the property of so modifying the beam of light as to effect its passage through the second or upper prism, and thus to the eye, neglecting for the time being those crystals inactive in this respect. This affords us a most ready means of discriminating between crystals and glass. The glass of the microscopic slide has no action on the light, neither has a purely glassy piece of rock; but let there be crystals present, and they are immediately seen, illuminated on a black ground. Viewed in this way by polarised light, the darker spherules of the obsidian become bright, while the rest of the rock appears black.

Leaving the glassy rocks, we may now glance at some specimens in which crystallisation has still further asserted itself. The rock called Felsite is one which, instead of being glassy, is micro-crystalline in structure: the crystals are very small, but their optical behaviour leaves no doubt as to their nature. There are many rocks which contain, imbedded in a base of this kind, larger, but still microscopic, crystals. Figs. 3 and 4 are examples of such. The crystals, though so small, are often wonderfully perfect.

Fig. 3 is a sketch of a micro-porphyritic basalt, from Unkel, on the Rhine; the largest crystal occupying the centre of the field is one of felspar, several smaller crystals of the same shape may be detected, the remainder of the work being micro-crystalline. The next figure gives a

section of a fine crystal of nepheline, in Phonolite basalt, from Sellberg Eisel. The crystal in shape is a hexagonal prism, and has been cut almost at right angles to its base. During its growth it has enclosed within it other small crystals and part of the base of the rock. Round the

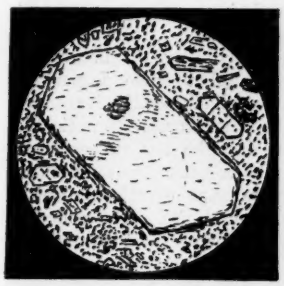


Fig. 3.

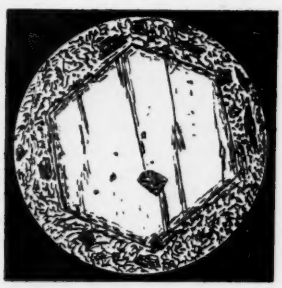


Fig. 4.

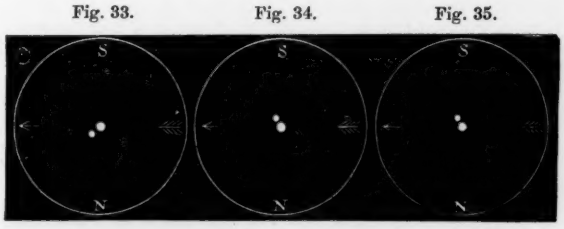
outer edges this may particularly be noticed: it would seem as though the power which determines the crystalline form, at the last, had just sufficient energy to complete the shape of the crystal; but, with the growing rigidity of the cooling mass, lacked the strength to expel these foreign particles, and was perforce content to enclose them within itself.

NIGHTS WITH A THREE-INCH TELESCOPE.

BY "A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.'

TO the east of Boötes lie the constellations Corona Borealis and Serpens, which we shall now proceed to examine. Beginning with the former (which really does present more than the ordinary resemblance to the object whose name it bears), we shall find a very interesting double star in ζ (map, Vol. I., p. 626), the components exhibiting well-contrasted colours. Its aspect, as seen with a power of 160, is shown in Fig. 33. σ Coronæ is a very pretty pair indeed; it is delineated in Fig. 34, as viewed with the same power as the last star. σ will be found in the sky as nearly as may be 10° N.E. of α Coronæ. This is sometimes ranked as a triple star, as the pair shown in our sketch are followed, at a distance of 51″ or 52″, by a minute blue star. σ itself is one of what are known as binary stars—*i.e.*, physically connected pairs; and, in the description of their orbits, about their common centre of gravity, its com-

Fig. 33. Fig. 34. Fig. 35.

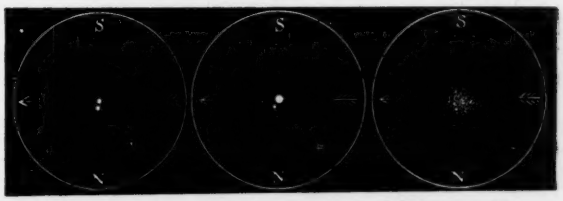


ζ Coronæ Bor. σ Coronæ Bor. δ Serpentis.

ponents have separated from 1″.3 in 1830, to something like 3″.5 now. One of the most interesting of these binary systems, that of η Coronæ, is unfortunately quite hopelessly beyond the power of our instrument, as the two stars are now less than 0.5″ apart, and are closing. Their distance varies from about 1.4″ to 0.3″, and their orbit is described in something over forty years. There are several pairs of telescopic stars in this constellation, all of them tolerably easy to divide, but it is very difficult to give directions for finding them in the absence of an equatorially-mounted telescope with divided circles. An easy one (Struve, 1,964) will be found a little to the south-west of ζ described above. While going over Corona, the student should not omit to glance at that most astonishing object, T Coronæ; the star which blazed up suddenly as a second magnitude one in the year 1866. Examined by our greatest English spectroscopist, Dr. Huggins, on May 16 in that year, it was found to exhibit a double spectrum; one analogous to that shown by our own sun, the other one that of glowing gaseous hydrogen; this (possibly) indicating a conflagration on a stupendous scale. Subsequently to this the star faded to the 9th magnitude, revived again somewhat, and has since been irregularly variable. At present it appears as a star of about the 9½th magnitude. It is situated on an imaginary line drawn from ϵ Coronæ to π Serpentis, at rather less than one-third of the distance between the two from ϵ.

Serpens, to which we shall next devote our attention (Map, Vol. I., p. 626), is one of those straggling and sprawling constellations so difficult to follow in the sky. Nevertheless it is one containing many beautiful and interesting objects. To begin with, α is a very wide and unequal pair, the smaller component requiring a good deal of looking for with a small telescope. We insert it here for the pretty contrast in colours which it presents. δ Serpentis, shown in Fig. 35, is a very neat and pretty binary star. The components are at present separating. β is, like α, a widish and very unequal pair, the small star, as in the former case, being bluish. θ Serpentis is comparatively wide and easy. It will well repay examination from the richness of the region in which it lies. ν Serpentis, 4° north-east of η, is also wide and easy. As before, we mention it for the pleasingly-contrasted colours of its components. 5 Serpentis (9° south-west of α) is much closer, and very unequal. It will repay examination. 10½° to the north-east of α Serpentis, on a line drawn from that

Fig. 36. Fig. 37. Fig. 38.



49 Serpentis. 59 Serpentis. 13 M. Herculis.

star to Vega, will be found 49 Serpentis—a fine pair, shown in Fig. 36. This is a binary system, with a supposed period of 900 years! 59 (or d) Serpentis is a beautiful object, the colours of its close and unequal components being strongly contrasted. It is represented in Fig. 37. Smyth's directions for finding this star are, perhaps, as good as any. "To identify 59 Serpentis," he says, "let an east-south-east ray be shot from β Herculis through α, which will be found two-fifths of the way" (*i.e.*, from β Herculis to 59 Serpentis).

Libra (Map, Vol. I., p. 626) is neither a striking constellation to the naked eye, nor does it contain many objects accessible to the class of instrument we are employing. A small, but easy, pair of stars will be found in No. 62 of Piazzi, XIVth hour. It lies 15° east by north of Spica Virginis, or 2½° south-west of ι in the same constellation. 9° due west of β Scorpii will be found P. XV. 91, a not very

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close, but considerably unequal pair. ι Libræ is a very wide and unequal pair, but worth looking at for its prettily contrasted colours. Just to the north-west of 5 Serpentis, of which we have previously spoken, will be found that fine, compressed cluster of very small stars, No. 5 of Messier's Catalogue. It is scarcely resolvable in a 3-in. achromatic, and merely appears like a nebula, brightening conspicuously towards the centre.

We now arrive at that somewhat unintelligible constellation, Hercules, who appears on the maps and globes head downwards between the constellations of the Northern Crown and the Lyre. As our present object, however, is less to endeavour to reconcile the configuration of the stars composing this constellation with the counterfeit presentment of an inverted hero, than to select from them curious and beautiful objects, suitable to the instrument we are employing, the map we use will supply all the aid necessary for this purpose. We say all the aid; but, in truth, the map which should give the position of a quarter of the interesting objects with which this constellation teems, would have to be a very elaborate and crowded one indeed. We must then, perforce, confine ourselves to a few of the most easily identifiable. Beginning upon the confines of Corona Borealis, about $1\frac{1}{2}°$ a little to the east of south of ρ Coronæ, we shall find 23 Herculis. This is a wide pair, but we insert it here for the marked colour of the smaller star, which will be seen below, and just to the right, of its primary. ζ Herculis, a remarkable binary star, is quite beyond the power of our telescope—in fact, appears single with the means at our disposal. If, though, we fish along a line connecting η and ζ Herculis about one-third of the way η, we shall light upon an object which will amply repay us for any disappointment we may experience in connection with this. The object to which we refer is No. 13 of Messier's Catalogue, and consists of a most glorious globular cluster of stars. How far we shall succeed in detecting its stellar character will depend upon the excellence of our instrument and the acuteness and training of our vision. We have tried to indicate its character in Fig. 38. One-and-a-half degrees N. by E. of η Herculis will be found another cluster (Messier 92), which the average eye and instrument will only show as a bright nebula. We may further note here that there are two planetary nebulæ in this constellation; but that only one of these is at all within the reach of a three-inch telescope; and neither can be found with certainty, save in an equatorially-mounted telescope.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART IX.

IF kept standing in the dark slide for any length of time, the sensitive collodion film will dry, and the time it will remain in a condition fit for use will depend on the temperature of the air. Another disadvantage is that the silver solution required to form the picture while under development drains away, and if the weather be very hot, and the corners of the dark frame are not clean, markings, known as "oyster-shell," are likely to form, and often spoil the negative. Two or three folds of red blotting paper wetted and placed at the back of the plate will retard evaporation and keep the film sensitive for a considerable time, and if the plate have lost much of the silver, it may be restored by redipping in the silver solution. But after all, the results on plates so preserved are seldom satisfactory.

Various methods have been adopted for keeping the plates in a moist state, such as coating with solution of sugar, honey, glycerine, or other suitable vehicle that will retard the drying of the film. It is obvious that if the sensitiveness of the film could be retained in a *dry* state, we should have a convenient substitute for the wet process. The labour involved in working with wet collodion away from home conveniences, the necessity for some kind of tent, the possibility of breakages, and occasionally the discovery that some important article has been left at home — all these disadvantages set many minds working to discover a practicable *dry* process. To enumerate or describe the processes at various times introduced would be tedious. Most of them, in the hands of their discoverers, gave excellent results, and some of them came into general use. The collodio-albumen process may be named as one of the best, and has been used very extensively. It has the disadvantage, however, of being very slow, but the results, in the hands of careful workers, have been quite equal to wet collodion. It will be sufficient to say here that the plates are prepared, first, with bromo-iodized collodion in the usual way. On the removal of the plate from the bath, it is washed and then put into a weak solution of iodide of potassium and again washed. The plate is then coated with albumen, containing iodide and bromide of potassium in various proportions, and also liquor ammonia. It is then dried by a fire, and is ready to be sensitised, or rather to be re-sensitised, which is effected by immersion in a bath of nitrate of silver in the ordinary way, and it is again washed. The plate is then allowed to dry without the application of heat. The plates will keep in good condition for many months, and the latent image may be developed at pleasure, and after the lapse of many weeks or months. The development is effected by using pyrogallic acid, citric acid, and nitrate of silver in certain proportions; and hyposulphite of soda is used for fixing the pictures.

I have described this process in general terms first, because it gives most excellent results, and should not be passed over; and, next, because the amateur, if he desire to make use of it, will seek particulars in treatises devoted to the subject.

Another method by which the silver bath may be dispensed with is by adding nitrate of silver to the collodion, forming an emulsion which may be poured on to the plate in the ordinary way, and when dry is ready for use. The development is effected by the alkaline method.

The practice of out-door photography may be said to have been entirely changed during the last three or four years by the introduction of the *gelatine dry plates*, and not only for out-door work, for these plates have now almost superseded collodion and the silver bath in the studio work of the professional photographer—at least for portraiture. The extraordinary rapidity of the gelatine film has effected this result. As compared with collodion, the time of exposure necessary to obtain a picture has been reduced to the fraction of a second; so rapid, indeed, is the action of light upon the plate, that, for out-door work, the effect is as instantaneous as the exposure can be made by mechanical means, and for studio work the change may be said to be ten or twenty times less than by the old method.

In a future paper we may return to the subject, and describe the method of preparing the gelatine plates. To do so now would be unprofitable to the tyro, owing to the delicacy of some of the operations. The plates can be purchased ready for use, of various degrees of sensitiveness, from five to twenty times that of wet collodion, and, if desired, this degree of sensitiveness may be exceeded;

but for all ordinary purposes, ten times the rapidity of wet collodion is quick enough and much to be preferred. If the plates are very rapid, it becomes extremely difficult so to time the exposure as to obtain the best results—over-exposure resulting in weak negatives—and a weak gelatine negative cannot be so easily intensified as one of a similar kind taken with wet collodion.

As the manipulation of the gelatine plates is quite different from the wet collodion, and as some little experience will be necessary in order to obtain good results, we would suggest that the $4\frac{1}{4} + 3\frac{1}{4}$ plates be used. They can be purchased, of excellent quality, at from eighteenpence to three shillings per dozen. In the next paper we shall describe some of the methods in general use for developing the gelatine plates.

HOW TO GET STRONG.

INTRODUCTORY.

FROM youth to middle, and often to past middle, age, most men are apt to be too closely engaged in the struggle of life, to pay due attention to the strength of the body. They may take daily what they consider a sufficient amount of exercise; but the exercise is not calculated to keep the various limbs and muscles, still less the internal organs, in proper working order. Amid the ordinary concerns of life the man may appear strong, even stalwart. But when occasion arises for some special muscular exertion, or taxing the action of some organ, he finds out his weakness. A sudden alarm may tell him that his heart is weak. A sharp run of two or three hundred yards, or even less, may show him that his lungs are not to be trusted. A walk rather longer than he is accustomed to, and taken at a sharper pace than usual, may leave him so tired during the next day or two as to show that his lower extremities are not in good working order. Or he may learn a similar lesson from a day's rowing or tricycling. In many more subtle ways, however, which often only an experienced doctor can recognise, the imperfect condition of the bodily mechanism, internal as well as external, may be indicated.

Now, in the present day, the men who are of most value to the community cannot afford time to acquire skill in athletic exercises. A man whose energies are wanted in his profession, be it law, medicine, literature, or the church, or who has to attend keenly to business matters, either in his own behalf or in trust for others, might gain in some respects by a course of training such as would suit a prize-fighter, a wrestler, a runner, or a long-distance walker, but he would lose a great deal more. The energies cannot be heavily taxed in different ways, each sufficing to employ them fully, without bankruptcy of health. If, then, a man gives his energies in large part to bodily training, he cannot, without ruinous effects, devote his mind sufficiently to the business of his life, which, we assume, *not* to be the achievement of great feats of strength and endurance.

It is useless, then, or worse than useless, to endeavour to show members of the general community how they can become athletes. They have not time for the necessary exercises, and if they had time they would be most unwise to use it in this way. But men, and women too, though they may have no occasion to acquire skill in athletic exercises, have great occasion to possess sound bodies,—unless they are passing absolutely useless lives, when they may do as they please, so far as their value to the community is concerned. Even then, they add greatly to their own comfort and happiness by exchanging flabby muscles, too sensitive nerves, imperfect breathing apparatus, and feeble circulation, for a body healthy throughout,—the *corpus sanum*, without which the *mens sana* is impossible.

We propose in the series of papers of which this is the introduction, to show how, by devoting a few hours weekly to well-arranged exercises, this end can be attained. No violent exertions are necessary, no difficult feats need be attempted, no special form of exercise need occupy much of the time and attention; but each day a well-directed plan is to be followed, by which the weak and untrustworthy parts of the body are to be found out, and then steadily improved by exercise, until finally the body becomes like the "one-hoss shay" in Wendell Holmes's ingenious parable:—

— — In building of chaises, I tell you what,
There is always *somewhere* a weakest spot,
In hub, tire, felloe, in spring or thill,
In panel or cross-bar, or floor or sill,
In screw, bolt, thoroughbrace—lurking still,
Find it somewhere you must and will,—
Above or below, or within or without,—
And that's the reason, beyond a doubt,
A chaise *breaks down* but doesn't wear out.

What is true of chaises is true of animals, man included. What the worthy deacon did for his new "shay" we can do for our bodies, or come near to it. Said the deacon:—

. . . . . 'Tis mighty plain
That the weakes' place mus' stan' the strain;
'n' the way t' fix it, as I maintain,
Is only jest
T' make that place as strong as the rest.

This is what we have to do with the vehicle in which we travel the road of life, if we would avoid premature collapse.

THE VICTORIA INSTITUTE.—Captain F. Petrie, Honorary Secretary of the Victoria Institute, noticing a remark in our Answers to Correspondents which appeared to reflect on the scientific position of this Society, has forwarded to us some of the recent papers read before the Institute. From these we learn, to our regret, that we have done the Society injustice in assuming that its aim and scope are the same now as they were for the first few years after it was founded, when alone we were acquainted with its work. We gladly render this *amende*, rejoicing to see that a Society which includes so many able and eminent men among its members, now devotes itself chiefly to the advancement of truth and knowledge, not, as of yore, to the demolition of theories deemed hostile to particular religious views.

ENGLISH.—In a kindly paragraph in the *Journal of Education*, reference is made to our answer to a correspondent who had asked for some work on the Queen's English, explaining that he wanted a book dealing with composition, figures of speech, &c. We replied: "There is a small book on the Queen's English by Abbott, head master of St. Paul's; forget publisher's name and the price." On this the *Journal of Education* remarks that "a little knowledge is a dangerous thing, though in a scientific editor such ignorance is excusable." If our friendly critic refers to our momentary forgetfulness of the publisher's name and the price of the work, he is right enough: but he would hardly speak of that as ignorance. It occurs to us he may possibly imagine that we attributed to Dr. Abbott Dean Alford's work on "The Queen's English." We were not, however, referring to that work at all. It would not have suited our correspondent, any more than would Mr. Washington Moon's "The Dean's English." Either would encourage an affected and, so to speak, priggish style of writing. (We speak from rather remote recollection, as we have seen neither later than 1866.) Our reference was to the excellent little book, "English Lessons for English People," by Dr. Abbott and Professor Seeley, published by Messrs. Seeley, Jackson, & Halliday—a work of a very different character. Readers of our Answers to Correspondents must remember that they *are* answers to questions, and cannot always be fully understood by others than the questioner. In this case, we should certainly not have used the expression "the Queen's English" had not our correspondent used it, as it is one to which on principle we strongly object.

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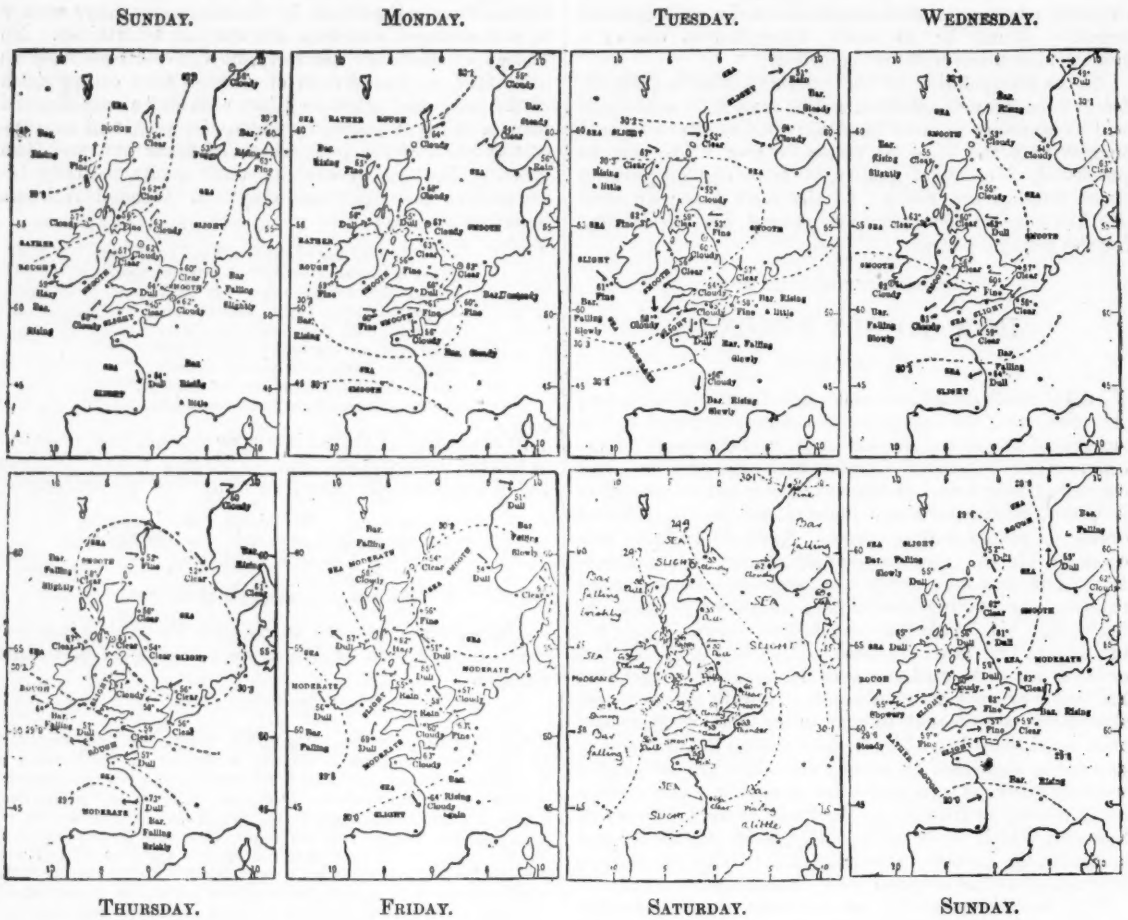
WEATHER CHARTS FOR WEEK ENDING SUNDAY, JUNE 4.



In the above charts the dotted lines are "isobars," or lines of equal barometrical pressure, the values which they indicate being given in figures at the end, thus—30·4. The shade temperature is given in figures for several places on the coast, and the weather is recorded in words. The arrows fly with the wind, the force of which is shown by the number of barbs and feathers, thus:— ⟶, light; ⟶, fresh or strong; ⟶, a gale; ⟶, a violent gale; ⊙ signifies calm. The state of the sea is noted in capital letters. The * denotes the various stations.

BUTTERFLIES AND MOTHS.

By W. J. H. Clark.

ALL the insects we mentioned last week will still be on the wing, and probably they may be supplemented by a few others, the principal of which are the Gold Swift (*Hepialus Hectus*), the Four-dotted Footman (*Lithosia Mesomella*), the Little Thorn (*Epione Advenaria*), the small White Wave (*Asthena Candidata*), &c.

Caterpillars of all kinds are very plentiful at this time of the year, though many require to be very diligently sought for, on account of their colour or some other peculiarity.

None of the Hawk moths are in the larval state at present, but the caterpillars of the Green Forester (*Ino Statices*) are to be found on the sorrel and thrift, those of the Six-spotted Burnet (*Anthrocera Filipendulæ*) on the crow-foot trefoil, the Muslin (*Nudaria Mundana*) on tree lichens, the Red Arches (*Calligenia Meniata*), or the oak, beech, and birch; the Common Footman (*Lithosia Complanula*) on the lichen attached to poplars; the Common Tiger (*Chelonia Caja*) on the nettle, oak, and numerous other plants. This, the commonest of British caterpillars, is to be found everywhere and under numberless names, it being the "woolly bear" of country children.

The Brown Tail (*Liparis Chrysorrhœa*) and Yellow Tail (*L. Auriflua*) on the oak, elm, blackthorn, &c.; the Satin (*L. Salicis*) on the poplar and willow; the Gipsy (*L. Dispar*) on the elm, oak, lime, &c.; the Lackey (*Bombyx Neustria*) on the pear, apple, &c.; the Five-spot Burnet (*B. Trifolii*), or the bird's-foot trefoil, plantain, &c.

Among the Geometræ there are the Canary Thorn (*Ennomos Tiliaria*) on the oak, birch, lime, &c.; the Pale Brindled Beauty (*Phigalia Pilosaria*) on the oak, whitethorn, &c.; the common White Wave (*Cabera Pusaria*) on the birch; the Mullein Wave (*Acidalia Promutata*) on the nettle, yarrow, &c.; the Common Wave (*Cabera Exanthemaria*) on the sallow; the Common Heath (*Fidonia Atomaria*) on the birds'-foot trefoil; the Currant Moth (*Abraxas Grossulariata*) on the currant, sloe, &c., together with many others.

Several of the Noctuæ have reached the caterpillar stage now, among which we may mention the Small Wainscot (*Nonagria Fulva*) on the sedge, the Dusky Brocade (*Mamestra Furva*) on the grey hair grass, the Coast Dart (*Agrotis Cursoria*) on the spurge, the Garden Dart (*A. Nigricans*) on various low plants, the Autumnal Rustic (*Noctua Glareosa*), the Hebrew Character (*Tæniocampa Gothica*) on the broom, lilac, &c., the Clouded Drab (*T. Instabilis*)

on the oak, willow, &c., and the Common Quaker (*T. Stabilis*) on the oak and elm.

The most favourite foods of caterpillars are oak, elm, beech, elder, nettle, &c., and a search among these is sure to give good results; and, with a little perseverance, a good bag ought to be made. It is difficult at first to tell to which species the larva belongs, but practice and time are the only roads to knowledge.

To make a good cage or breeding-house for caterpillars requires but a slight acquaintance with carpentry, and with a few tools, and perseverance and patience, something very tolerable can be turned out. One of the favourite cages is of the form of a box (see Figs. 1 and 2), made with quarter-inch pine, with a portion of each end cut away to admit pieces of very finely perforated zinc (Fig. 1A), to be nailed or otherwise fastened in. The front is also cut away, to allow a sheet of glass (Fig. 2B) to be inserted, through which the light can pass, and the movements, &c., of the caterpillars be seen.

Fig. 1.—End View.

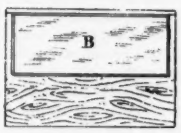


Fig. 2.—Front View.

In the back is a door by which the occupants and their food are put in. On the bottom is placed a layer of damp earth or sand, about two inches thick, in which to stick the twigs or plants used as food, the moisture keeping them fresh for a longer period than if placed on the dry board; it also serves for those larvæ which form their cocoons in the earth, if kept soft and open. The size of the cage is immaterial, but as caterpillars do not thrive without plenty of room, it is as well to have it as large as convenient; about two feet long by one foot wide, and fourteen inches deep will be found very good dimensions.

Many other varieties of breeding-houses are used, but not one is such a general favourite as the above; as make-shifts, however, any ordinary box, or a flower-pot covered over with book-muslin, very fine wire netting, or perforated zinc can be used; but the trouble of making a proper cage is amply repaid by the success obtained in the rearing of larvæ.

FEEDING.—Always feed the caterpillars once a day, with fresh leaves, gathered, if possible, in the early morning. Very young larvæ require leaves which have not arrived at maturity, as the full-grown ones are too tough for their mandibles. To change larvæ from the stale to the fresh food requires great care in the manipulation; giving the twig a sharp jerk will often detach some, whilst to remove more obstinate individuals, a fine camel-hair pencil must be used. If, however, any should refuse to quit with either of these means, the best plan is to put them with both old and new food together in the cage, where they are sure to find their way to the fresh leaves.

EASY LESSONS IN BLOWPIPE CHEMISTRY.

BY LIEUT.-COLONEL W. A. ROSS, LATE R.A.

LESSON VI.—THE PHYSICS AND CHEMISTY OF THE THING (*continued*).

IT may be supposed—nay, is supposed by the "pooh-poohing class" of chemists and physicists in England—that any investigation as to the cause of the Blowpipe pyrocone formation, when all we have to deal with is its results, must be superfluous; but that this is not the case, is shown by the obvious mistake into which even such a shrewd observer as the famous Swedish chemist Berzelius fell, from neglecting the former. He says (page 18, Whitney's translation, page 28 Children's), "Formerly, the blue flame was considered as the proper one for the reduction of oxides, but this idea is erroneous; it is in reality the brilliant part of the flame which produces deoxidation," &c. If Berzelius had been aware of the facts mentioned in Lesson V., he would never have penned such a sentence as the above; besides which, modern competent blowpipe operators are well aware that there is no such thing as "a brilliant part" of a perfect blowpipe pyrocone.

I regret to have to add to this part of the physics or pyroconics of blowpipe operations, that many of my conclusions being, unfortunately, opposed to those of some of the most distinguished English chemists of the present day, have remained unread and unnoticed for the last ten years, on all shelves containing the "Proceedings of the Royal Society," condemned to obscurity by what Mr. Crookes calls (in reviewing my smaller work in the *Chemical News* of May 28, 1880) "the conspiracy of silence," until the Germans chose to take the matter up the other day. For instance, Professor C. L. Bloxam, F.R.S., Professor of Chemistry in King's College, London, says ("Bloxam's Chemistry," third edition, page 98):—"It must be remembered that, in using the blowpipe, the stream of air is not propelled from the lungs of the operator (where a great part of its oxygen would have been consumed) but simply from the mouth, by the action of the muscles of the cheeks." In my work "Pyrology," I ventured (with the deference, of course, due to the opinion of such a celebrated chemist) to appeal to Professor Huxley and Dr. Carpenter against this account of what may be called "the physiology of blowing."* I stated that I considered it impossible for a man (or any other animal) to collect air in his mouth for blowing or other purposes, except by means of his lungs, in the ordinary function of respiration, unless, indeed, we suppose him to replenish his inflated cheeks by gulping pure air into his mouth in a kind of process of fly-catching, although a similar representation seems to have been made in the time of "Hudibras," who also denies its possibility, Part II., Canto I., page 179:—

> "As no man can draw in his breath
> At once, and force out air beneath."

But, as usual, no notice whatever was taken of my protest, and I am, therefore, compelled to bring it forward again in the downright pages of KNOWLEDGE seven years afterwards.

2. As regards the second phenomenon† mentioned in Lesson V., the auriferous P. acid bead described in Lessons III. and IV. affords one of the best examples I know, of a substance assuming different colours whilst very hot and slightly hot, to that it retains when "cold," or when its temperature has fallen to that of the surrounding air. My pupils may remember that these colours were "brilliant topaz-yellow, when very hot; then, in cooling, green; then greenish-blue; and lastly, when cold, a beautiful blue-violet." These are the principal chromatic‡ indications afforded to the unassisted vision of an ordinary observer by this bead whilst cooling, but in such a rapid process, the intermediate transitions from each colour to the other must necessarily pass unobserved. Now, when I discovered in 1869 that gold could be made to afford a coloured bead by means of the blowpipe (a fact utterly unmentioned in all previous writings on chemistry or blowpipe analysis), I noticed that these indicating colours of the cooling bead were those of the solar spectrum§; that the changes invariably took place in the same direction, viz. from the red end of spectrum to the violet end, or, in Newton's language, from the end containing the "least refrangible" (or bendable) to that of the most refrangible rays; and thirdly, that a bead containing bismuth, dissolved in it, instead of gold, cooled in the other chromatic direction; that is, it also was topaz-yellow hot, but instead of changing to green, like the gold bead, that it became paler and paler in cooling, until it ended by being colourless (or white), and, if very much bismuth was dissolved, reddish-orange when cold. I tried bismuth, because I knew that that metal, unlike nearly all the others, expands in cooling. Now, most of us know generally that colour is nothing but the absorption‖ and reflection of certain coloured eliminated rays, from white light by certain substances—a kind of analysis¶ of white sunlight, performed, for instance, by the violets and roses, &c., around us, by which some of the coloured rays composing white light are absorbed or concealed from, and others reflected or presented to, our vision.

I feel quite sure that most of my pupils will now have guessed the drift of these experiments. Most metals, gold among them, expand, that is, their particles or "molecules" are separated more widely from each other than before, by heat. They become, in fact, more porous. It seems obvious, therefore, that in a solution of a metal, which is the finest state of division of its particles yet known, the alteration caused in the size of these pores by heat (although far beyond the powers of the most powerful microscope) would be such as to enable certain coloured rays to pass into the hot body, which could not pass into it when cold, and to be there absorbed. The pores gradually closing as the bead (or gold solution) cools, exclude the rays of light which they formerly admitted.

* Greek: *Phusiologia*. The science which treats of vital functions.

† Greek: *Phainomenon*, an apppearance.

‡ Greek: *Chromatikos*, coloured.

§ Latin: *Spectrum*, an appearance. Newton's name for the coloured image afforded by his prism on the wall.

‖ Latin: *Absorbeo*, I drink in.

¶ Greek: *Analusis*, the separation of a body into its constituent part.

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absorbing others only; and this goes on until the body has attained the temperature of the surrounding air, when there is no further alteration of porosity or of colour.

This solution (I don't mean the gold solution, but the solution of a problem which, so far as I can ascertain, had not been before investigated) seems simple enough to have been, like the rest of my work, "pooh-poohed" by some of our too-authoritative chemists, who invariably deal with inquiries so inconvenient as not to bring themselves prominently before the public gaze, in the spirit of the famous Johnsonian reply to Boswell, "Dont ask me 'why,' sir! it is sufficient for you that it is so;" but, fortunately for me, a Mr. Ackroyd—who was introduced to me at the South Kensington chemical laboratory (soon after my book "Pyrology" was published in 1875, when I found a copy in the library there) by the late Assistant Professor of Chemistry, as being interested in these researches of mine with regard to the change of colour in hot substances cooling,—read a paper on the subject before the English Chemical Society, in which he not only makes no reference whatever to my unfortunate book, but says, "Hence when, at an early stage in its study, I saw that nearly all metachromatic * changes take place in a definite order—the order of the spectrum colours—I was under the impression, even after much reading" (too much pyrological reading I fear!) "that the subject was quite unworked." The chemical society, however, whose attention was attracted to the matter by an article in the *Chemical News*, very properly refused to allow Mr. Ackroyd's paper to be published in their proceedings; and I repeat, this was a fortunate circumstance for myself on the whole!

(3) Reasoning from analogy† to the former case (2), we may assume as an hypothesis, that this curious phenomenon—the production and abolition of colour in solutions of oxides by their different positions with reference to the blow-pipe pyrocone—is caused by an alteration of the texture of the material of experiment, which then admits different colour rays to those it previously did. This seems likely from the fact that nearly the same colour can be produced in solutions of perfectly different oxides, by placing them in particular positions with reference to the pyrocone. Thus a bluish violet can be produced in a manganiferous P. acid bead by O.P.; in an auriferous bead by a half-inch P.P.; and in a cobaltiferous bead by H.P. But the whole of this question requires working out pyrologically, and when it has been so dealt with, it will, I believe, afford some interesting and important evidence as to the real nature of what we call "oxides."

The solubility of silica in P. salt (sodium metaphosphate) and its insolubility in P. acid before the blowpipe are, of course, due to the presence of the large percentage of soda present in the former. Berzelius showed (and it is one of the most interesting and still important parts of his book on the blowpipe) that silicic acid (or silica) fuses B. B. effervescently, with a certain proportion of soda to a transparent bead, but only "forms a slag" with more; that is, forms a vitreous mass which is not transparent. He even added tables of the solubility thus, of several siliceous minerals to the fourth and last edition superintended by him in 1844, before his death in 1845, so that he evidently considered the matter important. At the same time he decided that microcosmic salt has a wholly acid action on substances B. B.; which the solution of pure silica in it, by the application of greater heat, most decidedly shows that it has not. Silica, being an acid itself, has naturally no "affinity" for the P. acid in which it is treated, and can obviously only form sodium pyrosilicate (which is fusible) when a sufficiency of soda is set free in the bead for that purpose. The 10 per cent. of soda in "glacial phosphoric acid" is *not* sufficient for that purpose, so the silica remains wholly undissolved. An interesting question is thus raised whether the application of greater heat by volatilising phosphoric acid sets free a proportionate quantity of soda; or whether it directly increases the intensity of the alkaline part of the re-agent?

LECTURES BY MR. PROCTOR.—In the interests of KNOWLEDGE, I had intended to discontinue lecturing for a few years. But the bantling begins to run earlier than I supposed it would walk; and having received complimentary requests from Birmingham, Bristol, and other important centres that I would reconsider my determination, I have decided to do so.

THE June number of the *American Naturalist* contains a biographical notice of the late Professor Darwin, by Dr. Packard, and the articles are devoted almost exclusively to the subject of evolution.

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* Greek: *Meta*, change. Mr. A.'s expression, therefore, may be taken to *mean*, "a change of colour changing."

† Greek: *Analogia*, an agreement or likeness between things.

THE BABYLONIAN CALENDAR.

AT a recent meeting of the Society of Biblical Archæology, an important paper was read by Mr. Theo. G. Pinches, of the British Museum, describing several calendars from Assyria possessed by that institution. Of these there are two which are duplicates, almost entire, and a third, which, unfortunately, only contains the first four months—Nisan, Iyar, Sivan, and Tammuz.

The principal peculiarity of these calendars is that they furnish for each day statements as to whether it is lucky or unlucky, together with injunctions as to what should be done or avoided on many of the days, and, finally, a number of prognostications of astronomical events, chiefly eclipses, precisely as we foretell them in our modern almanacs.

With regard to the astronomical portion of their contents, what appeared the most extraordinay statement they give is the prediction in several cases of a solar eclipse on the 20th of the month, because, if the Babylonian months as generally considered previously were lunar ones, this is obviously impossible. Supposing the translation to be correct, of which there can be no doubt, the words being "eclipse (heaven darkness) of the sun," it insists upon one of two conclusions, either that the advanced knowledge of astronomy in Babylonia has been much overestimated, or such clearly impossible predictions would never have been made; or else, that the theory of the Babylonian months being lunar ones must be abandoned. With regard to their acquaintance with the true laws of eclipses, a study of the series of astronomical inscriptions, as far as they have been published, sufficiently shows that whilst they were perfectly capable of foretelling, with but very slight error, those of the moon, they were frequently sadly at fault when dealing with those of the sun. It was the custom for officers situated in different parts of the kingdom, or in tributary territories, to send accounts of eclipses to the monarch at the capital, and from what appears in these "report tablets," as they are called, it seems that sometimes, either from having with the army corps an astronomer who knew when eclipses were anticipated, or because notice had previously been sent out from the observatories at Babylon or Sippára, a careful watch for the phenomenon was kept. For instance, the late Mr. Fox Talbot translates one of these tablets thus. After the usual preliminary courtesies and invocation of Divine blessings which commence an address to a royal personage, the writer says:—"The army sent into Egypt from my lord the king has arrived. In the month of Su (Tammuz) an eclipse happened, my soldiers, for the welfare of Assyria in their hearts were concerned, to the right and left they watched the heavenly portent. The bearer of these letters let the king inquire (from him) the circumstances of the eclipse." Another tablet, deciphered by Mr. George Smith, says: "Concerning the lunar eclipse, of which the king sent me in the cities of Accad, Borsippa, and Nipur, the observations they made, and in the city of Accad we saw part, the observation was made, and the eclipse took place .... and when we made the observation for the eclipse of the sun, the observation was made, and it did not take place. That which I saw with my eyes, to the king, my lord, I send." Then follow some astrological omens connected with the lunar eclipse, which are said to portend evil to the Syrians and Hittites, but good to the King of Assyria; most likely a piece of discreet official flattery. In another case, an astronomer, named Nebo sum Adin, reports to the king as follows:—"On the 14th day of the month we kept watch upon the moon, and the moon was eclipsed." On the other hand, another *savant*, Abil Ishtar, says:—"On the 28th, 29th, and 30th day, a watch for an eclipse of the sun, we keep the sun behind, the eclipse did not pass." Another tablet tells us:—"The moon out of its reckoned time midway comes, an eclipse it makes." A solar eclipse of the time of Assurbanipal, which M. Oppert, in a paper recently presented to the French Academy, identifies with that of June 24, B.C. 661, and which was visible in Susiana, is said to have happened in the evening of three successive days. That any writer should care to chronicle such a report as to an event in a neighbouring country, shows their knowledge of solar eclipses was so imperfect as to leave ample margin for error.

From these few specimens of their astronomical literature, it may be gathered that whilst their mastery of the true theory of lunar eclipses was practically perfect, that of those of the sun was as yet in embryo. The classic author Geminus tells us that the lunar cycle of 223 lunations, or a little more than eighteen years, was discovered by the Babylonians. Ptolemy says they had a continuous series of observations of lunar eclipses up to 747 B.C., and that of this table the five earliest derived from it by Hipparchus were B.C. 747, 720, 621, and 523. It does not, however, follow from this that they did not ascend even further back still, as undoubtedly was the case, for even in the scanty remnant of the official archives already disinterred, we find the eclipse of B.C. 763 recorded. As to solar eclipses, the cause of their ignorance is obvious, for although the lunar cycle applies equally to that of

the sun, even if their astrologers understood this they could find no recorded law of recurrence of solar eclipses within that period by which to predict them. Because, though they recur in fixed order within the cycle, they are not visible at the same places on their repetition, as where first observed, whereas all lunar ones are, of course, to be seen at all parts of the earth having the moon above the horizon. That the Assyrians ever arrived at the truth is very questionable, although Professor Sayce seems to think that the problem of calculating solar eclipses by tracing the shadow as projected on a sphere had presented itself to them; for, when we first hear of a correct prediction of a solar eclipse, it is by a man who gained his wisdom not in the astrological observatories of Assyria, but in the sacerdotal colleges of Egypt.

The quotations already given (which might easily be multiplied) are ample to prove that whilst the Babylonians were far from perfect in their calculation of solar eclipses, yet they so thoroughly understood those of the moon, and were generally so far advanced in astronomy, that they knew much better than to predict an eclipse of the sun on the 20th of a lunar month. In fact it is evident that their months must have been so arranged that these phenomena could take place upon any one of the days. For instance, in what are termed "Portent Tablets," translated by Prof. Sayce, are numerous lists of omens arising from lunar eclipses on such days as the 13th, 14th, 15th, 16th, 20th, 21st, &c. It does not follow from other days not being mentioned that eclipses were not anticipated on them, because their omission may be owing to parts of the portent tablets being wanting; or, their occurrence upon these other days, not being associated with any omen, are unnoticed. Mr. Sayce tells us that records of eclipses for every day of the year were kept. In the "Cuneiform Inscriptions of Western Asia," vol. III., p. 56, published by the British Museum, we have lists of events to be apprehended if an eclipse happened on any day of the month Tammuz up to the 15th. Elsewhere we read—"In Tammuz, from the 1st to the 30th, if an eclipse happen the altars are destroyed," &c.; and, again, "In the month Sivan, from the 1st to the 30th, an eclipse failed, the crops of the land are not good;" and, as a final instance, this curious sentence may be given relating to an unexpected eclipse—"If, in the month Tammuz, an eclipse happens, which has not been calculated, the heart is established." That the disappointment in the one instance, and the unexpected good fortune in the last, refer to miscalculated *solar* events is evident from what has been said above.

It only remains to be ascertained what was the length of the Babylonian months, and to this there is now no doubt that the correct answer is, as stated by Prof. Sayce, that they were months of thirty days,* as were those of the time of the flood (Gen. vii., 11; viii., 3, 4); thus their year would be one of 360 days, corresponding with the Egyptian vague year. That this is the true state of the case is evident, because we know that every six years an extra month was intercalated, the Ve Adar, in order to make the year coincide with the solar year. The year of 360 days being five short, in six years the correction was made, because then the error amounted to a month. This, however, was not sufficient to entirely rectify the difference, being about thirty-five hours in error, and in 124 years the deficiency would amount to a whole month of thirty days, and so we accordingly find another intercalary month mentioned in the inscription, the "second Elul." Now, whilst a 360 day year is about five days less than the solar, it is nearly *six* days more than a lunar year; thus in about five years the days of the month would advance through a whole lunar month, just as in six years they would retrograde a whole month as compared with the solar year. The effect of this would be that in a period of five years eclipses could and would take place on any day of the month. A curious confirmation of this is to be found in the report tablets consisting of records of careful observations for the new moon on the 1st and 30th days of the month; these, doubtless, being accounts of observations made at the end of the five year periods to see if the error of six days annually had rectified itself, and the new moon once more coincided with the 1st of the month. The Egyptians, it is well-known, rectified the (360 day) vague year, with the solar, by intercalating five days annually; but there is reason to think this practice did not exist in the earliest times, because each of these five days is sacred to [one of the great gods, which seems as if it were a plan of the priests introduced with the innovation in order to render it permanent.

It is not supposed that these explanations clear up all the difficulties of the Babylonian calendars, because there is reason to believe that sometimes one of the intercalary months was a second Marchesvan, and Mr. Pinches finds evidence of there occasionally, in some sense, being intercalary days; but they are offered in the hope that they may be a preliminary step towards arriving at the truth.

JOSEPH OFFORD.

* M. Lenormant ("Ancient History," 451) argues that the months, like those of the Jews after the time of Moses, were alternately of twenty-nine and thirty days; practically lunar ones, but two of those to which he assigns only 29 days, Iyar and Marchesvan are mentioned in inscriptions, translated by the late Geo. Smith as months of thirty days.

SIEMENS ON SOLAR ENERGY.

WE have received from "X, University Club, Dublin," a letter respecting Dr. Siemens' scheme of the solar energy. The letter is marked "private," but how an anonymous writer can claim that his letter should be regarded as a private communication, we fail to see. We therefore disregard the injunction.

Beginning with the remark that our argument from the light of the stars seems to dispose of Dr. Siemens' theory, "X" presents Dr. Siemens' dynamical argument as akin to the following statement:—

"If a globe (representing the sun and its rotating atmosphere) be made to rotate in the still air of a room, there will be produced an outflow of air all round the equator from which will result inflow at both poles."

And he adds, "Surely this proposition needs only to be stated to be accepted."

We accept all except the words in parentheses. The globe hanging in the still air of a room, and exerting scarcely any appreciable attraction on the air, does not and cannot represent the sun rotating in the atmosphere of space. The rotating sun cannot generate any such tangential velocity as could lead to equatorial outflow,—the rotating globe in a room can.

The fundamental error of Dr. Siemens' position is the assumption—in which X follows him—that gaseous matter outside the sun's rotating atmosphere, will yield to the influence of any centrifugal tendency which may be communicated to it, but is not under the influence of solar attraction.

The same fallacy underlies, I suspect, the following assertion (I suspect *only*, because X does not tell us what he really means):—

"You have only to reconsider the statement about 'the pressures which could exist opposite the polar regions [of the sun], and *the consequent resistance to inflow*,' to see that the underlined words must not stand."

Of course, if, as X and (manifestly) Dr. Siemens suppose, the interplanetary atmosphere were free from solar attraction, and the sun were like a ball spinning in the still and apparently uniform air of a room, the underlined words could not stand. As matters are otherwise, they can, and do. If X will examine the conditions as they would actually exist (the interplanetary atmosphere being assumed), he will see that, whether the pressures opposite the polar regions are at any moment in excess or in defect of those which would result in equilibrium, there can never be other than oscillatory movements in the interplanetary atmosphere. There will always be resistance to inflow—that is, to the constant inflow, which the theory requires: there will equally be resistance to [constant] outflow.

RICHARD A. PROCTOR.

WINNING WAGERS.—It will hardly be believed, but our paper thus named has actually been understood by some of our readers as a guide to successful wagering! It is as painful to have to interpret sarcasm as to have to explain a joke; but for the benefit of those (very few we trust) who have misunderstood us, we explain that the whole aim and purport of our paper was to indicate the rascality of the only kind of wagering which is ever systematically successful,—the system pursued by the bookmaking fraternity. We might as reasonably be supposed to inculcate the true principles of pocket-picking, if we warned readers against the tricks of street thieves, as to advocate the true principles of wagering, when we show how bookmakers swindle verdant bettors.

KILLING ENTOMOLOGICAL SPECIMENS.—In the column on "Butterflies and Moths," p. 606 of KNOWLEDGE, several methods of killing the animals are described. Some twenty years ago, I tried many experiments with this intent, and finally discovered that the bisulphide of carbon is effectual. It is a very volatile liquid, its vapour very dense. The insects, either before or after pinning out, may be placed in a wide-necked bottle or a tin box in which is a piece of wool that has been slightly wetted with the bisulphide. It should remain there a few minutes, as a shorter time only produces temporary insensibility. I described this method in the ninth volume of *Nature*, and it has since been extensively used in France for the destruction of phylloxera.—W. MATTIEU WILLIAMS.

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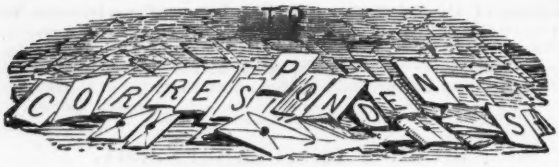


Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

*** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

TOBACCO AND CONSUMPTION.

[418]—In letter No. 411, page 630, Mr. W. B. Wicken proposes the use of tobacco as a preventive and cure for consumption.

The fact that the Turks are comparatively free from consumption is, I believe, beyond doubt; and it may be, though I do not think so, that their habit of inhaling the smoke into the lungs keeps away that dreadful disease. On the other hand, how is it that consumption is so prevalent in our own country? We are a great nation of smokers, and if we do not make it a point of inhaling the smoke into our lungs, a fair percentage of it must necessarily find its way to the organs of breathing. Personally, I do not place any faith in tobacco as a cure for consumption, for I believe that climate has more to do with it than anything else. As a preventive it *may*, possibly, be of service, by keeping the cells of the lungs open, and thereby prevent the accumulation of morbid secretions. Tobacco smoke has, undoubtedly, some soothing effect, and allays irritation of the lungs; for I find that when my laboratory is filled with acid fumes, that I have no inclination whatever to cough, as long as I use my pipe.

With regard to the tobacco used by the Turks, I may say that I have never smoked a stronger kind than that used with the nargillah. When on a visit lately to Constantinople, I one day ordered, out of curiosity, a cup of coffee and a nargillah. The latter, however, soon proved too much for me, though the smoke was drawn through three yards of tubing and about six inches of water. After passing through so much, the smoke was still so powerfully noxious, that one puff satisfied me fully, and I was glad to resign the pipe to a stander-by, who finished it in happiness. The tobacco used with the pipe is different from that used for cigarettes.

In Spain, too, the smokers draw the smoke into their lungs, and will frequently engage in a very spirited argument before emitting it again. They seem to have no difficulty whatever in swallowing the smoke—in fact, it is their usual custom: but their tobacco is not unlike chopped hay in mildness.

In conclusion, I will repeat that Englishmen consume a great deal of tobacco, and yet consumption is very prevalent amongst us. I believe that it is to our changeable and humid climate that the cause must be assigned, and this applies not only to consumption, but to all the diseases that the chest and lungs are subject to. Perhaps some of our medical contributors will give us their experience in this matter, which I am sure is of great interest to many. I would just say one thing, and that is, that rather than believe the Turk's freedom from tubercular diseases is due to the manner in which they use tobacco, I should say it is because of their abstinence from intoxicating liquors.

W. O. PROSSER, F.C.S.

THE LATE MR. DUNMAN.

[419]—The recent death of Mr. T. Dunman has already been noticed in your columns. He may be said to have sacrificed his life to his self-denying zeal in the work of popular education, and to an intense interest in the teaching of Science. Will you allow me to mention that a fund is being raised to help his widow to place herself in a position to support herself and her two children? Several members of the Council of the Working Men's College are on the Committee, and I am authorised as treasurer to receive contributions.—R. B. LITCHFIELD, 4, Bryanston-street, Portman-square, W.

TRICYCLES.

[420]—In reply to H. R. L., I fear I can add little to the information I have tried to give in my two articles on recent tricycles, which have so lately appeared in KNOWLEDGE, but if the writer will ask for details of any particular machine he may find wanting in what I have written, I shall be very glad to furnish the information to the best of my power. JOHN BROWNING.

MILDNESS OF THE SEA COAST.

[421]—The effect of the near neighbourhood of the ocean in moderating cold seems to be well illustrated by comparing the minimum temperature for Valentia, given in your weather reports for three weeks, ending April 29, with those observed at this station—Killarney—distant inland from Valentia 50 miles.

The minima here were *lower* than at Valentia on those 21 days as follows:—(The amount is given in whole degrees)—5, 8, 4, 2, 1, 0, 6, 8, —1, 4, —1, 1, 2, 2, 0, 1, 6, 8, 6, 1, 1.

The mean difference is 3°. This is a somewhat striking exemplification of the well-known effect of the moderating influence of the presence of the sea. I may add that the instrument used here has a Kew verification. It would be, I think, an improvement to give the *hour* of the barometer readings in your Weather Reports.

G. R. WYNNE.

THE "COLD SNAP" IN MAY.

[422]—Permit me to suggest as a reason for the low temperature of a week in May that there is usually formed in that month a region of high barometric pressure in the north, causing prevalent easterly winds over England. Frequently this wind is light and anticyclonic, and, after a period of cloudy skies, there is a clear sky for several nights. This, with the comparative calmness of the atmosphere, promotes rapid radiation from the earth. The ground temperature falls decidedly below freezing-point, and the air is cooled rapidly in the stratum next the earth.

I believe in this direction must be sought the cause of the cold snap. Later on in the season the amount of heat received from the sun in its longer period above the horizon is sufficient to prevent the excessive loss by radiation experienced in May, even in the clearest calmest summer nights. G. R. WYNNE.

POPULATION OF THE EARTH.

[423]—On a globe 2 ft. diameter, the Dead Sea appears as a small coloured dot, but if it were frozen over there would be room on its surface for the whole living human race, allowing six square feet for each person, and were they suddenly to be engulfed, it would merely raise the level of the lake by some four inches.

One hears at times very wild statements as to the depth the earth would be covered if all the people who have lived were laid side by side on its surface, estimates running from four feet upwards; but if we suppose the earth to have been as populous as now for 6,000 years (200 generations), the whole number could have been decently buried in graves 5½ ft. × 2 ft. within the British Isles, and some 7,000 square miles to spare.

I have taken the present population, as given by the German statisticians, at 1,456 millions (nearly). W. W. M.

STAR SPECTROSCOPE—GLASS STYLOGRAPHS—MAGNIFYING POWER.

[424]—With a good 4-in. Cooke, provided with a Maclean's star-spectroscope, but mounted on an ordinary altazimuth tripod, without slow motions, the faintness of star-spectra, and especially the unsteadiness of the motion, made it extremely hard to determine the best focus. The way I succeeded is simple, and may be useful. Slide out the prisms and replace the concave cylindrical lens, fit the instrument as usual in the place of the eyepiece, focus on the Pole

star, till an intensely narrow and brilliant *line* of light is seen, and mark the focus obtained. Then, if the prisms are reinserted, very good definition is obtained in the brighter parts of the spectrum. I have thus been able to see a wonderful amount of detail in the spectrum of Arcturus, and the general character of the spectrum of a star of the fourth magnitude. Several scratches thus made in the dark on different occasions do not differ in position $\frac{1}{10}$th of an inch; of course the focus will differ considerably with different people, but with the same observer it suffices perfectly to set the instrument by the mark on each occasion, and saves a great deal of fatigue and annoyance.

I noticed a remark in KNOWLEDGE, No. 21, that glass tubes for ink writing are very hard to make, and break easily; as I have habitually used glass-tube pens of my own make for every kind of writing, including this, during the last two years, I venture to differ. Perhaps it depends on the way they are made; but most people who use mine think them superior to the ordinary stylographs (12s. 6d. each). Anyone can make them for himself if he knows the way.

Apropos of the magnifying power of a deep eyepiece; a microscope with a micrometer on the diaphragm *in the eyepiece* will measure the power of the deepest lens very accurately and quickly. I tried it on the posterior lens alone of a 300-power eyepiece, and it gave 411 diams. as the result; the image of the object-glass was so sharp that it could be easily thus measured to $\frac{1}{3000}$th of an inch.

H. L. C.

SCREW-DRIVERS.

[425]—May I suggest, through the medium of KNOWLEDGE, that the simplest and cheapest method of improving screw-drivers, would be to make the heads of all screws concave, and the ends of all screw-drivers convex.

With a little care at first, the driver would never slip off the screw, and one could always see if the screw were going in straight.

C. CARUS WILSON.

[I have long been of opinion that instead of a single cut in the screw-head, two cuts forming a cross would be better, the screw-driver being made to match.—ED.]

THE MOUNTAIN "HERCULES."

[426]—E. C. R. (413) is surprised that no expedition has been sent to verify Captain Lawson's discovery of the mountain "Hercules" in New Guinea. The reason is the same as that which has stood in the way of a verification of Captain Gulliver's discoveries of Laputa, &c. Every traveller who has had any experience in mountain climbing knows how to treat an account of an ascent which commenced at 4 A.M. at the base of the mountain, which completed the first 14,000 ft. by 9 A.M.; then reached the snow-line at a height of 15,000 ft., and the summit, 10,314 ft. above the snow-line, and temperature 22° below freezing, at 1 P.M. The book is altogether a hoax, and a very feeble one. It was snuffed out immediately it appeared. W. MATTIEU WILLIAMS.

Answers to Correspondents.

****** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of KNOWLEDGE, the increasing circulation of which compels us to go to press early in the week.***

HINTS TO CORRESPONDENTS.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 4. Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

J. L. M. Certainly; you would be most unwise not to insure your life, in a good office. We propose presently to publish in KNOWLEDGE a series of short papers on Insurance.—M. DOUBLEDAY. The lottery method which you describe gives a perfectly fair and equal chance to all concerned. It has the disadvantage that some might get two prizes; that might be obviated by arranging that, when a given number was drawn a second time, the drawing should be cancelled.—SIGMA. Possibly some of our readers may be able to tell us about Whitechapel needles (*re* Witchcraft), white witches, &c.—CLOUD. Such clouds are always spoken of as *cumulus* clouds, not *cumuli*.—E. D. GIRDLESTONE. Many thanks. You will see I have retained your quotation unchanged.—C. J. L. Lord Dundreary used to consider that puzzle, about the increasing number as we go back to great grandparents (great)^2 grandparents, &c., to (great)^n parents. The true answer is that not a few thousand million-billions, but the whole of your large number, except a few hundred millions, must be struck off for marriages of cousins, &c., for we are *all* cousins, if men came from a single stock.—A CHURCHMAN. Pardon me; Darwin never taught—no man of science ever has taught, or could (when in his senses) teach—that there is nothing more than a First Cause setting the earth in motion and then leaving it. Darwin slightly (in reality, though to our ideas vastly) extended our recognition of the domain of law,—outside remains infinity. Biology teaches that there is a Cause setting an egg in existence, and that then, under natural conditions, the egg becomes a chick, which developes into a fowl. There is as much in this to shake your faith in Deity, as there is in the development of a world,—in other words, there is *nothing*. When you say that the truth and authority of God's Word is an established fact, you assert a truism. God's word must be true. God's works must be true also. We may misunderstand these; may we not also misunderstand those. Moreover, we are certain that everything that is, is God's work (though the very conception of a God and of His mode of working may be quite outside our powers); we may not be quite so certain that this or that book, or chapter or verse of any book is God's, word. If you are "A Churchman" of one denomination—it matters nothing which—you assuredly accept as such *some* writings which churchmen of other denominations reject, and *vice versâ*. In one of the books which you probably (but how can I *know?*) regard as God's word—at any rate, it was and is so regarded by the Jewish people, and has been accepted as such by a large portion of the non-Jewish world—you will find that one whose words are there spoken of as specially approved by God, says that, "As touching the Almighty, we cannot find Him out," meaning, I take it, that we cannot by observation or experiment recognise the specific working of something outside the laws of nature. Darwin taught this—or, to speak more truly, he *was* taught this by observation and experiment. If you assert that he was wrong, you must be prepared to show on what grounds you except that particular book, or chapter, or verse, from those which a great part of the religious world regard as God's Word.—T. G. FULWOOD. Let e be the modulus of elasticity; M the momentum lost by moving ball (A), and gained by the ball (B) which was at rest, during contact and compression; then the momentum generated by the force of recovery after impact $= e$M; and the total momentum lost by A and gained by B $= (1+e)$M. Therefore, if v' be the velocities of A before and after impact, u the velocity of B after impact, we have (if m is the mass of either ball)—

$$mv' = mv - (1+e)\text{M} \qquad (1)$$
$$mu = (1+e)\text{M} \qquad (2)$$

Now if the balls were inelastic, and w their common velocity after impact, we should obviously have—

$$mv = 2mw; \text{ or } w = \frac{v}{2}$$

$$\text{and } M = m(v - w') = m\left(v - \frac{v}{2}\right) = \frac{mv}{2}$$

and as M is the momentum generated before elasticity comes into play, it is the same in value whether the balls are elastic or inelastic. Hence, substituting in (1) and (2), we have—

$$mv' = mv - (1+e)\frac{mv}{2} = \frac{mv}{2}(1-e)$$

$$\text{or } v' = \frac{v}{2}(1-e)$$

$$\text{and } u = \frac{v}{2}(1+e)$$

If the balls are of great elasticity, or e very nearly equal to unity, v' the velocity of A after impact is very nearly evanescent, and u the velocity of B after impact is very nearly equal to v. In other words, in this case the moving ball is brought very nearly to rest, and the other, which had been at rest, advances with very nearly the velocity which the moving ball had had. Thus, if both balls are of glass, for which substance the modulus of elasticity $= \frac{8}{15}$, the moving ball will have its velocity reduced to $\frac{1}{32}$nd part of what it had been, and the other will have communicated to it a velocity of $\frac{31}{32}$nds of the velocity originally possessed by one ball alone. —BON ACCORD. Are not the words "Art is long" aptly associated with the reference to the shortness of life? The lesson is to do what we can each for our special art, remembering that time is fleeting,

> And our hearts, though stout and brave,
> Still like muffled drums are beating
> Funeral marches to the grave.

M. STRONG. I fear your question whether Italian was derived from Latin or Latin from Italian would hardly suit KNOWLEDGE. There is an interesting discussion of the origin of Italian, French, Spanish, &c., in Hallam's "Middle Ages." It seems likely enough that Latin was a special form of a language spread over Western

XUM

Europe, long before the time when Latin literature began—just as the English of our best authors is a special form of a language spread over Britain and parts of Europe, long before English literature began. Rome could never have forced her language on so many nations.—J. V. M. Thanks. There are passages in Dr. Draper's works which are warm, but I have seen none unfair. The other writer you name (with whom, however, I thoroughly sympathise), has been at times a hard hitter. It is very gratifying to know that you find KNOWLEDGE free, as I have wished it to be, from sly thrusts.—W. SAUNDERS. (1) For lectures on the moon nothing can be more suitable than photographic slides, say from Nasmyth's Moon, used either with the oxyhydrogen light or a good stereoscopic lantern. (2) Probably, a submerged thicket; but careful survey would be required to determine the point.—W. S. No; the word was correctly enough printed "barometer." Prof. Huxley did not, however, imply that the barometer is highest on hot days, but that on the hotter of two days of equal atmospheric pressure, and when therefore the barometer should be of the same height, the mercury stands a little higher in the barometer tube. This is correct, but not for the reason he assigns. It is the diminution of the specific gravity of the mercury, not the change in the bore of the tube, which causes the difference.—A READER AND AN ADVERTISER. Thanks. I quite agree with you about having the articles as popular as possible. I scarcely see how they can be made much more popular without becoming trivial. Of course, some, as in our mathematical column, are not meant to be popular, but are for the benefit of special classes of readers. But we always give a fair supply, and rather more, of popular matter.—J. L. WHITAKER. We gladly insert your letter.—W. SHAW HAYLER. It was unquestionably a sunspot. You do not mention the power you used.—M. C. C. It would not be easy to explain a real objective change in the colour of sunlight from golden to pure yellow during a partial eclipse. The effect was, I should think, subjective; there was a real diminution of light, and an illusory loss of colour.—T. MORGAN. Sorry your letter has remained so long unanswered; but the truth is, we get about three times as much correspondence as we can deal with, consequently some of it is four weeks old. If you send a short letter expressing your views respecting pyrological matters, we will gladly insert it.

THE TELESCOPE.

A. B. C. There are only two Nebulæ, 10 M. Ophiuchi and 12 Messier Ophiuchi, in the neighbourhood of which you speak. As, however, the former is very nearly *on* a line joining μ Serpentis and δ Ophiuchi, and produced as far again, while you say definitely that the object you observed was above such a line, there can be but little doubt that it really was 12 M. that you saw. This will be referred to in due course when Ophiuchus comes to be treated of in the "Nights." Doubtless the reason why "F.R.A.S." ignored α Cassiopeiæ as a double star was that its companion is more than 1′ 30″ from it, so that they are really two separate and distinct stars, and in no legitimate sense whatever a pair or a double one.—W. H. HARRIS. The simple numerical formula for calculating the focus of a lens equivalent to the two in a Huyghenian eye-piece is this:—Divide twice the product of their foci by their sum. Let us apply this to your own eye-piece, of which you say that the field lens has a focal length of $\frac{9}{8}$ths of an inch, and the eye lens a focus of $\frac{3}{8}$ths of an inch; then multiplying these together (according to the precept), and the product by 2, we get $\frac{27}{8}$ by 2, or $\frac{54}{8}$. Next we take the sum of the foci $\frac{12}{8}$, and dividing $\frac{54}{8}$ by $\frac{12}{8}$, we get $4\frac{1}{2}$; so that $\frac{4\frac{1}{2}}{8}$, or $\frac{9}{16}$ inch is the focus of a single lens of equal power to your combination. Finally, dividing 36 inches, the focal length of the object glass, by $\frac{9}{16}$, the focal length of the eye-piece, we obtain 64 as the magnifying power of the latter with that object-glass.—J. SMITH. The amplifying power of a Barlow lens depends upon its position in the cone of rays from the object-glass. If we call the distance of the lens from (*i.e.* within) the focus of the object-glass d; then if its negative focal length be 2 d, it will exactly double the power of every eye-piece. In other words it would give an amplification of 160 with your 80 eye-piece and of 800 with the 400 one. In your case this result would obviously be attained if you had a Barlow lens of 8 inches (negative) focus placed 4 in. within the focus of your object-glass, or, in other words, 56 in. from it.

[About three pages of "Answers" have been unavoidably held over.]

Our Mathematical Column.

THE LAWS OF PROBABILITY.

IT seems better to give the most general law for inverse probabilities before proceeding to deal with illustrative examples. This we proceed to do:—By extending the reasoning employed in the last paper, the reader will have no difficulty in seeing that if there are three or more bags, each containing the same number of balls, of which p in the first bag are white, q in the second, r in the third, and so on; then, if a bag is selected at random, and a ball drawn at random proves to be white, the chance that the first bag was selected is $\frac{p}{p+q+r+\&c.}$; the chance that the second was selected is $\frac{q}{p+q+r+\&c.}$; the chance that the third was selected is $\frac{r}{p+q+r+\&c.}$; and so on. Nor will he have any difficulty in making the requisite modifications where the several bags contain different numbers of balls. The method to be followed is precisely the same as I employed in the simpler case of two bags, and the result is similar, viz., that if the chance of drawing a white ball from the first bag is C_1, that of drawing a white ball from the second bag C_2, that of drawing a white ball from the third bag C_3, and so on; then, if a white ball is drawn from a bag selected at random, the chance that the first bag was selected is $\frac{C_1}{C_1+C_2+C_3+\&c.}$; the chance that the second bag was selected is $\frac{C_2}{C_1+C_2+C_3+\&c.}$; and so on.

This result, extended to the more general case of which the bags of balls are merely illustrative, becomes the following general law:—

If there are three or more hypotheses all equally likely, and mutually exclusive, so that only one can be true, and if on the first hypothesis the chance of an event is C_1, on the second the chance of an event is C_2, on the third C_3, and so on; then, if the event occur, and we know, further, that it must have resulted from some one of the conditions inferred by the hypotheses, the chance that the first hypothesis is the true one is $\frac{C_1}{C_1+C_2+C_3+\&c.}$; the chance that the second is the true one is $\frac{C_2}{C_1+C_2+C_3+\&c.}$; and so on.

One step further, and we have the most general law of all. The above law supposes all the hypotheses to be equally likely in the first instance—a state of things obviously corresponding to the equal chances that any one of the bags will be selected. To illustrate the case where the hypotheses are not equally likely in the first instance, we must assume that the chances of drawing the several bags are not equal. Now, if we consider the case of two bags, we shall be able to deduce the general law we require. Thus, suppose there are two bags, and that the chance of selecting the first is $\frac{2}{5}$; that of selecting the other being $\frac{3}{5}$ (for, since one or other must be selected, the sum of these probabilities must be equal to unity); and, as before, let the chance of drawing a white ball from the first bag (if selected), be C_1, that of drawing a white ball from the second bag being C_2. Then we may represent these two chances by supposing that there are two bags of the first kind and three of the second kind, all equally likely to be taken; for it is obvious that the chances that the selected bag belongs to the former or latter kind are, respectively, $\frac{2}{5}$ and $\frac{3}{5}$. Now, by the general law already obtained—if a white ball is drawn, the chance that it came from a specified bag of the first kind is, $\frac{C_1}{C_1+C_1+C_2+C_2+C_2}$, or $\frac{C_1}{2C_1+3C_2}$, and the chance that it came from one or other of these two bags is $\frac{2C_1}{2C_1+3C_2}$. Similarly, the chance that it came from one or other of the three bags of the second kind is $\frac{3C_2}{2C_1+3C_2}$.

Noticing how this result has been obtained, and proceeding at once to the law which the bags of balls illustrate, we obtain finally this general law, including all the preceding laws (of indirect probabilities):—

If an event cannot happen unless some one of a set of hypotheses, H_1, H_2, H_3, &c., be true (these hypotheses being mutually exclusive), the antecedent probability of H_1 being c_1, that of H_2, c_2,

and so on; and if on the hypothesis H_1 the chance of the event happening is C_1, on the hypothesis H_2 the chance of the event happening is C_2, and so on; then, if the event happen, the chance that H_1 is true is

$$\frac{c_1 C_1}{c_1 C_1 + c_2 C_2 + c_3 C_3 + \&c.};$$

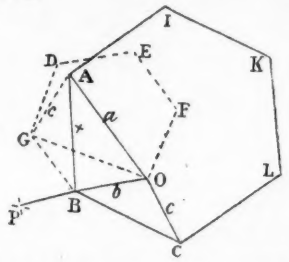
the chance that H_2 is true is

$$\frac{c_2 C_2}{c_1 C_1 + c_2 C_2 + c_3 C_3 + \&c.};$$

and so on.

(To be Continued.)

SOLUTION OF Y'S PROBLEM.—Upon the line OB, taken = b, construct the n-sided polygon, OBGD, &c. (in the figure $n=6$). Join the two alternate corners, O and G, by a diagonal OG. Upon OG as a base construct the triangle GAO, making the two sides OA and GA = a and c respectively, and taking care that A and B lie on opposite sides of OG.



Join AB, and on it construct the polygon of n-sides ABCLKI, which is the polygon required. For

By construction OA = a, and OB = b; also the triangles GAB and BOC are equal; because GB = BO, AB = BC, and angle GBA = CBO (each being equal to the interior angle of the n-sided polygon diminished by the angle ABO). Hence OC = AG = c. Q. E. D.

SCHOLIUM.—If the point P^1 be determined *on the same side* of OG with B, then the line BP^1 will be the side of a polygon fulfilling the conditions of the problem, except that the point O will lie *outside* of the polygon.

Our Whist Column.

BY "FIVE OF CLUBS."

PLAY THIRD HAND.

THE general principles which should guide the play third in hand are—First, and chiefly, to help and strengthen your partner as much as possible in his own suit; secondly, to derive all possible advantage from any strengthening card he may play in your own suit; and thirdly, to retain as long as possible such partial command as you may have in an opponent's suit. You can generally tell (from the lead, the play second hand, and your own hand) whether your partner has led from strength, or to strengthen you, or from a weak suit in which he has no means of knowing your position. This will be obvious from what we have already said about interpretation of the lead and the play second hand. Your play will be guided accordingly.

Let us begin, then, by considering the play third in hand: and afterwards, as heretofore, with the lead and second hand, we can consider the inferences to be drawn from the play of particular cards third in hand.

If you hold Ace third in hand, and neither King nor Queen, you play it in plain suits, unless King or Queen has been led. The only exception to this is when ten has been led and you hold Ace, Knave, when it is best to pass the ten. With Ace, King, you put on King, as a rule. But with Ace, King, and more than one small one, a good hand and four trumps, you should not put King on your partner's Knave, but pass the Knave; fourth in hand is sure to take the trick; but when trumps are out you are tolerably sure of getting in again, when you make two tricks at least, and probably three in the suit. On the other hand, with Ace, King, and small ones, but only three trumps, you should not pass the Knave. The principle here applied is general. With good strength in trumps, you may usually finesse with advantage in a suit in which your partner has led a strengthening card; but when short in trumps, such a finesse is unadvisable.

With Ace, Queen, alone or with others, you should finesse the Queen, or the lowest card in sequence with the Queen—unless a single trick will make or save the game, when, of course, such a finesse would be a Whist atrocity.

With King, Knave, &c., some players finesse the Knave. This is only right when you have such strength in the suit as to feel sure your partner has led a strengthening card from a short suit. It is nearly always wrong to finesse in your partner's suit. The finesse from Ace, Queen, &c., is almost the solitary exception to this rule. You should, therefore, in general play King, from King, Knave, third hand. When nine is led, and you hold King, Knave, and others, you pass the nine, if strong in trumps, and the state of the game is not critical (supposing always that second player has not covered the nine). Otherwise, you may either finesse the Knave, or not, as you may deem best. Fourth player is as likely to hold Ace and no other honour, as Queen and no other honour: if he holds both Ace and Queen, it is indifferent what you play; he will, in any case, remain with an honour over you.

(To be continued.)

A correspondent points out what, of course, I had noticed myself, that there is a double—really a single—error in my notes to the "Hand for Study," in No. 30. I had originally written out the hands and play from memory, being away from my books. I supposed B had three trumps, whereas in the actual game he had but two. Hence, in my notes, referring to second trick, I said B returns the highest of two cards left; and referring to third, A leads trumps again, though he is drawing two for one. Of course, when I saw the play of the hands, which had been taken direct from Cavendish, I recognised my mistake. But from some cause or other the necessary corrections were not made. The point of the game, however, is not affected by the mistake.

VALUE OF GOOD PLAY.—A correspondent asks whether good play really counts much at Whist, and describes several tests to which he has subjected the matter, with the result that good players have no better chance than bad ones. There is so much chance, unfortunately, in Whist as actually played (it might be immensely improved in this respect) that a casual observer, or one who watches play for only a few weeks, or even months, might very well suppose that bad players have quite as good a chance as the best players. But no one at all acquainted with the game practically can doubt that in the long run good play must invariably get the better of bad play. Not a Whist evening passes but a practised player will note half-a-dozen cases or more in which tricks—sometimes two or three at once—have been lost by bad play; while not more than one or two cases will occur during the same time, in which bad play has, by an accident, turned out well or good play ill.

Cavendish's experience should suffice—owing to its wide extent and carefully noted results—to settle this point finally and for ever. He tells us that of 30,668 rubbers played from January, 1860, to December, 1878, he won 15,648 rubbers and lost 15,020, and counting points, which tell far more, he won in all 85,486 points and lost 81,055, gaining thus a balance of 4,431 points. It is practically impossible that so large a balance in his favour should be due to mere chance. The difference must have been due to play. Were two good players matched in as many rubbers against two bad ones, the difference would be far greater.

Our Chess Column.

BY MEPHISTO.

VIENNA INTERNATIONAL TOURNAMENT.

After the finish of the 20th round on Saturday, June 3, the score stood as follows:—

Mackenzie	13½	Zukertort	11½	Tschigorin	8
Steinitz	13½	Hruby	11	Weiss	8
Mason	13	Wittek	9½	Bird	7½
Winawer	13	Schwarz	9	Ware	6½
Englisch	12½	Meitner	8½	Fleissig	6
Blackburne	12	Paulsen	8		

Dr. Noa has withdrawn from the Tournament. From the above score it will be seen that the English players are well to the fore, and we think that they will still more improve their position for final place.

On Thursday, June 1, Blackburne had to meet Steinitz, and the former won, he conquering his mighty opponent in only twenty-seven moves. Nevertheless, Steinitz has drawn well, he having reached Mackenzie's score, who had been leading, by personally vanquishing his rival for the time being, in their second game, which was played on Saturday, the 3rd inst. Steinitz's victory over Mackenzie will have an important influence on the final issue. Zukertort

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drew with Mackenzie and Winawer, but he defeated Mason, who is doing very well, as can be seen from the above score :—

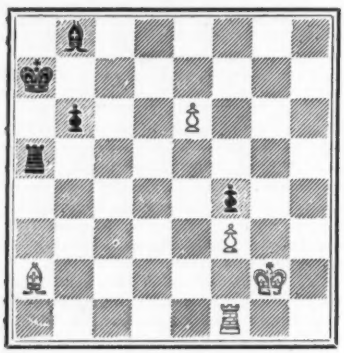
[*By Telegram.*] Vienna, Tuesday night.

The leading players are Steinitz, with 15, Winawer 15, Mackenzie 14½, Mason 14½, Englisch 13½, Blackburne 13, Zukertort 13, Hruby 11, Schwartz 10½, Wittek 10½.

We surmise from the above telegram that the second game between Steinitz and Zukertort must have resulted in a draw.

PROBLEM No. 45, End Game by B. Horwitz.

BLACK.



WHITE.

White to play and win.

1. P to K7 1. R. to K4

If 1. R takes B(ch), 2. R to B2 and the Pawn will Queen.

2. B to Q5 2. P to Kt4

White threatens mate by R to Rsq. If Black plays his B then R to Rsq(ch) and R8(ch) and Pawn Queens.

3. R to Ksq 3. R takes R

4. B to K4 and Pawn Queens and wins.

Game played in the Vienna International Tournament on May 14, 1882.

TWO KNIGHTS' DEFENCE.

White. Bird.	Black. Tschigorin.	White. Bird.	Black. Tschigorin.
1. P to K4	P to K4	19. Q to K2 (*f*)	Kt takes P
2. Kt to KB3	Kt to QB3	20. P to Kt3	Q to B4
3. B to B4	Kt to B3	21. K to Kt2 (*g*)	R to K3
4. Kt to Kt5	P to Q4	22. QR to Ksq (*h*)	QR to Ksq
5. P takes P	Kt to QR4	23. R to KRsq	P to KR4
6. B to Kt5 (ch)	P to B3	24. QR to KBsq	Q to Kt3 (*j*)
7. P takes P	P takes P		(*i*)
8. B to K2	P to KR3	25. Kt to Qsq (*k*)	P to K6 (*l*)
9. Kt to KB3	P to K5	26. B to Q4 (*m*)	Kt takes BP
10. Kt to K5	Q to B2 (*a*)	27. R takes Kt	Q takes P (ch)
11. Kt to Kt4 (*b*)	B takes Kt	28. K to Bsq	P takes R
12. B takes B	B to Q3 (*c*)	29. Kt to K3	P to QB4
13. P to KR3	Castles	30. B to B3	R takes Kt (*n*)
14. Kt to QB3	Kt to B5	31. P takes R	R takes P
15. P to QKt3	Kt to K4	32. Q to Qsq	P to R5
16. B to Kt2 (*d*)	KR to Ksq	33. B to Q2	P to R6
17. Castles	Kt f K4 takes B	resigns (*o*)	
18. P takes Kt	Q to Q2 (*e*)		

NOTES.

(*a*) This move is less attacking than Q to Q5, but it gives Black a good steady game.

(*b*) This defence is awkward; we prefer 11. P to KB4, B to Q3. 12. P to Q4, Castles. 13. P to B3, P to B4. 14. Kt to R3, P to R3. 15. Kt to B2.

(*c*) The Bishop occupies a good position on Q3; for the present it hampers White's development, and prevents his Castling.

(*d*) The Black Knight is very well placed on K4. White ought not to have hesitated any longer to withdraw his Bishop from its awkward position on Kt4 to K2.

(*e*) This gives Black a decided advantage.

(*f*) An effort to save the Pawn would have been unsuccessful; if, for instance, 19. P to B3, then Black plays B to B2, threatening Q to Q3, but if White replies to B to B2 with 20. Kt takes P, then Kt takes Kt. 21. P takes Kt, R takes P again, with a good game.

(*g*) A natural move; it enables White to oppose R on Rsq, should Black play Q to R4.

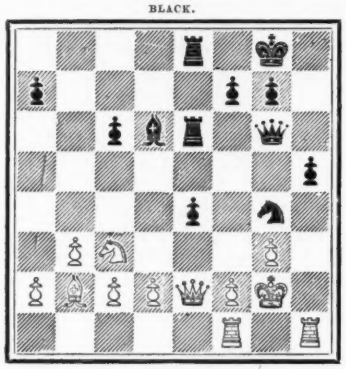
(*h*) We should have preferred 22. Kt to Qsq, to be followed by Kt to K3, or R to Rsq. QR to Ksq was not a defensive move; it would have been better to wait till the Queen's Rook was wanted; for, as actually occurred later on, this Rook played to Bsq, thus losing time.

(*i*) White has, as we pointed out in our former note, uselessly lost a move with his Queen's Rook.

(*j*) Black is playing with great tact; he initiates an ingenious combination, based upon the possibility of taking the Bishop's Pawn with the Knight, and then winning by taking the Knight's Pawn with the Queen, as she would be supported by the Bishop. We give a diagram of this interesting position :—

TSCHIGORIN.

BLACK.



WHITE.

BIRD.

(*k*) White ought to have seen the danger and played 25. R to R3.

(*l*) Highly ingenious, if now P takes P, then Kt takes P (ch), Kt takes Kt, R takes Kt, and wins. Of course, he dare not take with the Bishop's Pawn, on pain of Q takes P (mate).

(*m*) Although White's game is apparently hopeless, we should even now have preferred 26. R to R3, and, if Black replied, P takes BP, 27. Q to Q3, but it was only a question of which way to die. 26. P to KB4 would have been met by Black with P takes QP, 27. Q to Q3, R to K7 (ch), 28. Kt to B2, Kt to K6 (ch), &c.

(*n*) This brings matters to a speedy termination.

(*o*) As he cannot escape the ingenious mate prepared by Black, for if 34. B takes R, Q to Kt7 (ch), 35. K to K2, P to B8 Queens, with a double check and mate.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Correct solution of Problems Nos. 43 and 44 received from Francis J. Drake, C. W. Croskey, Clarence John Watson, Senex Solitarius, T. A. S., Berrow, Kit, H. A. N., G. W., J. B. of Berford, Moleque, B. G. Sergeant, Alfred B. Palmer, Leonard P. Rees, J. Bunyan, J. A. Schmucke, J. P., Herbert Jacobs.

No. 42 correctly solved by Brenton, C. W. Croskey, J. B., and Herbert Jacobs.

Novice.—Tell us where you stick; we will help you.

G. R.—We did not consider it necessary.

Kit.—In No. 351, Q to Bsq., 1. P takes P, and there is no mate.

NOTICES.

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